Systematic Zoology

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VOLUME 1 SEPTEMBER 1952 NUMBER 3

Systematic Zoology

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SYSTEMATIC ZOOLOGY is published quarterly by the Society of Systematic Zoology. Subscription rates are $4.00 to members, $7.00 to non-members and institutions. The price of single numbers, when available, is $1.50 to members and $2.00 to non-members. Annual dues are $1.00. Correspondence regarding subscriptions, membership, purchases, non-receipt, and other non-editorial business should be addressed to the secretary-treasurer of the Society, Dr. R. E. Blackwelder, Room 429, U. S. National Museum, Washington 25, D. C.

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Quantitative and Experimental Methods in Systematic Zoology

LEE R. DICE

SYSTEMATIC studies almost invariably begin with the collection of specimens which are taken as samples of certain populations. Attention must next be given to the amount of hereditary or nonhereditary variability within and between these populations. Another important part of the study will be the determination, so far as may be possible, of the amount of geographic, ecological, and genetic isolation between the several populations as they exist in nature. Often the investigator will wish to discover also the genetic and ecological factors which have directed the course of evolution in this particular group of animals.

Adequate Sampling

If a series of specimens collected in the field is really to be representative of a natural population, certain precautions about random sampling must be observed. The first question to ask is, What population actually is being sampled? Is it the population of a geographic race or perhaps of a whole species; is it the population occupying a county or township; or is it the population which inhabits a particular local meadow or grove of trees?

In the early days of systematic zoology it usually was sufficient to secure samples of a species wherever representatives of this species could be found. Such collections will generally serve to indicate the differences between full species. At the present time, however, most of the species, at least of the vertebrates, in the better-studied parts of the world are already well known. Attention is being given more and more to geographic races and to even finer divisions of species. It is well known that differences may occur between local isolated populations, and that a considerable amount of variation may occur within a small local population. In order that a collection of specimens may serve as a basis for the determination of the amount of variability within a species or within any of its parts, the individual specimens must have been taken at random and without a conscious or unconscious bias, such as might lead the collector to preserve only the largest or brightest individuals.

The prime requirement in random sampling, as opposed to haphazard sampling, is that every time a specimen is taken every individual in the population under consideration must have an equal chance of being chosen. It is usually impossible to locate and to number all the individuals in a wild population, in order that these numbers might be drawn from a set of numbered cards or from a table of random numbers. The requirements of random sampling will usually be satisfactorily met if the collecting is done with equal intensiveness and by equally unbiased methods in all parts of the area inhabited by the population. If it is impractical to collect in all parts of the area, then the sections in which collecting is to be done should be selected at random. This can be accomplished by dividing the area into subdivisions each of a size suitable for sampling, numbering these subdivisions in some manner, and then taking them at random. Not less than two random samples should be taken of each population, in order that the amount of variation from place to place within the habitat can be estimated. It is, of course, always desirable to have more than two samples.

For those species which occur as more or less separated local populations of varying size, modification of the usual random-sampling methods is necessary. The number of specimens taken from each population should be proportional to the size of that local population. It will rarely be practical to take a census of each local population in order to learn its size, but usually it will be possible to make a sufficiently accurate estimate of relative size on the basis of the area of continuous suitable habitat available to each population and on the basis of the richness of the cover and food supply.

After a sample of one particular population has been collected, the next step is to preserve the specimens. It will seldom be possible to study and to measure these specimens in the field. Limitations of time or space, and the cost of materials, often do not permit the preservation of all the specimens which have been captured. The manner in which the specimens are selected for preservation greatly affects their value as representatives of the population from which they were taken. For example, if all immature specimens are discarded while all the adults are preserved, the collection obviously would represent only the adults of the population. This may not be objectionable, so long as the basis for the selection of the preserved specimens is clear. On the other hand, should a selection be made among the adults, so that only the larger, more perfect, or more unusual specimens be preserved and the others discarded, then a definite bias is introduced and the collection will not be properly representative of the whole population of adults from which it was taken. If it is necessary to reduce the number of individuals to be preserved, then random-sampling methods should be followed.

It is unfortunate that most of the collections in our natural history museums have not been collected by random-sampling methods; consequently these collections are biased samples of the natural populations which they represent. With rare exceptions, therefore, museum collections are not suitable for the application of statistical methods. Few museums have adequate storage or curatorial facilities to care for the large number of specimens which would be needed to demonstrate adequately the range of variation in all the local populations of even a single common species. Nevertheless, it would seem that every museum could afford to collect and to preserve at least one mass collection taken by random-sampling methods for each species that occurs within the geographic area in which it is primarily interested.

I particularly deplore the practice of those museum curators who preserve only a single selected specimen of each species from each county within its geographic range and discard all the others. Such selected material may document geographic distribution, but it is not a satisfactory basis for the study of variation.

The troubles of the systematic zoologist will by no means be over even when he has available from various areas a series of mass collections, each taken according to the best random-sampling methods. It is axiomatic that he must standardize his measurements and avoid those which are subject to considerable instrumental or other error. He must give particular attention to the effects on his measurements of variation in sex, age, season, and special environmental factors, such as food shortage. In investigating the effects of age, he must consider those factors which affect one part of the body more than another, thereby producing differential growth.

One way of avoiding some of these difficulties is to restrict the comparison to individuals of the same sex and of the same age class. It may be assumed, for example, that most adult insects have reached their maximum size and, therefore, are comparable in their dimensions and other characters. Many mammals also reach an adult size, after which most of their measurements exhibit little

change. In *Peromyscus,* however, the individuals continue to grow long after they have reached sexual maturity. It is consequently never safe to assume that growth has ceased in a certain age class until this has been demonstrated to be true.

The identification of the age classes is difficult in many kinds of animals. In this respect the ichthyologists are fortunate, because in the scaly fishes they are often able to estimate the age of each individual from its scale characters. Moreover, the adult age class of most insects can be identified by the presence of wings. But in most other kinds of animals the age classes must be ascertained from such criteria as the body size, developmental stage, and sexual maturity. In the mammals it is possible to use molt stage, degree of fusion of certain bones, and the number of teeth and the amount of wear on them, as indicators of age. It is seldom, however, that any or all of these characters will give a completely reliable estimate of age. One must particularly guard against the fallacy of basing age class on such a characteristic as body size, of which the mean value and the amount of variability within the age class are later to be estimated.

Ratios between certain measurements are used by many systematic zoologists, particularly ichthyologists, in order to compare individuals differing in body size. This procedure is suitable, provided that there is no differential growth in the parts of the body which are being measured. Unfortunately, differential growth does occur in most animals, and consequently these ratios must be used with care.

Some systematists seem to believe that it is necessary to have large series of specimens in order to apply statistical methods to their measurements. On the contrary, statistics is of most use in small series, when one wishes to know whether or not the data are significant. The limitations of statistical methods must always be kept in mind, however. Statistical treatment will never give information that is more accurate than the original data, and in unskilled hands it may produce wholly unjustified conclusions. Nevertheless, when properly applied, statistics is a powerful tool with which every systematist should be familiar.

If all the individuals making up a given population measured exactly the same in all their characters, then a single specimen would provide all the information needed about that population. Actually, of course, variation occurs in the measurements of all populations. In order to estimate the amount of this variability, we must always have more than a single specimen. The actual number of specimens required as a basis for statistical calculation will depend, first, on the amount of variability in the population for the measurement under consideration, and, second, on the degree of reliability required in the estimate of the mean and of the variability.

On the basis of my own experience, and with due regard for the usual amount of variability in a biological series, for the frequent deficiencies of random sampling, and for the inaccuracies often inherent in the measurements themselves, I would say that a series of 100 comparable specimens is nearly always sufficient to give a very reliable mean and a satisfactory estimate of variability. It will seldom be necessary to accumulate measurements of more than 100 specimens, except where some special problem in variability is being considered. Fewer than 100 measurements will suffice for many studies. Fifty, twenty, or even a smaller number of specimens will often provide a satisfactory basis for statistical calculations. This will be particularly true if interest is centered mostly on the reliability of the mean, and if the amount of variability in some other related and more adequate series is already known. Any series of less than twelve specimens, however, will probably be unsatisfactory for statistical treatment. Even two specimens, of course,

is many times better than one, because at least some indication of the amount of variability in the population will be provided.

Here let me emphasize that no amount of statistical treatment of the most carefully made measurements will ever supplant the skill and understanding of the experienced taxonomist. I have often been amazed at the facility with which taxonomists thoroughly experienced in the study of a particular group are able to estimate the probable amount of variability in a population previously unknown to them. Careful measurements and statistical treatments can aid in the diagnosis of populations, but the interpretation of these figures must always depend upon the competence of the systematist himself.

Of all the systematic zoologists it is the paleontologists who have the greatest difficulty in securing random samples of specimens that will provide comparable measurements. Fossil specimens often are broken or incomplete. Also, it is seldom possible to secure any large number of fossil specimens which can be assumed to have come from a population whose individuals lived at the same time and at the same site. Nevertheless, Simpson (1944) has shown that the application of statistical methods to paleontological series yields results of great value.

Quantitative studies of series of museum specimens alone are not adequate to solve all of the problems that confront the systematist. For the solution of many problems in systematics, experiments under controlled conditions are required. Let me hasten to say that such experimental studies need not necessarily be conducted within a scientific laboratory; many valuable experiments can be done outdoors. It is a great deal more difficult, however, to control the various modifying factors in nature than in the laboratory.

Experimental Studies in Systematics

Some of the earliest experiments in systematic zoology involved the attempt to cross two species of animals. It has been known for a long time, for example, that the horse and the ass will cross, but that their hybrid offspring are nearly always sterile. Similar breeding experiments conducted in zoological parks, in laboratories, and in other controlled situations have given us much information about the relationships between particular species and about specific categories. Natural hybrids between species also are sometimes discovered living in the wild. It is not always possible to be certain of the parent species of such hybrids. Nevertheless, careful studies in this field by Hubbs (see Hubbs and Hubbs, 1932) and by others have contributed to our interpretation of the relationships between species in nature.

Even those species that can cross and produce fertile offspring often exhibit antipathies to one another that usually prevent hybridization between species in nature. Experimental studies by Dobzhansky (see Dobzhansky and Mayr, 1944) and by others are demonstrating that this sexual isolation is an important factor in maintaining the distinctness of species. Similar studies are needed in other groups of animals.

A related but much neglected line of research is concerned with the selection by animals of their habitats. We know very little about the precise factors in the habitat which stimulate animals to choose the sites where they are to live, nor do we know much about the psychological reactions through which animals select their homes. Careful laboratory and field studies of habitat selection are greatly needed.

Every series of specimens of any one species, irrespective of where it has been collected, exhibits more or less variability in each of its characters. Workers who have not had experience in the examination of series of specimens often fail to appreciate the extent of this natural variability within populations.

Variability in specific and in racial characters is known to be produced both by variations in heredity between the indi-

viduals and by the effects of the environment on the development of the characters. Rarely will it be possible to evaluate correctly from museum specimens alone the relative roles played by heredity and by environmental factors in producing variability in any given population. A number of years ago I heard a physiologist declare that the systematists were wasting their time in describing geographic races, because in his opinion most of the variability within species was due to the direct effects of the environment. Such a statement was of course absurd, for it has long been known that many of the characters of human races and of the strains of domestic animals are not greatly altered by transfer from one type of climate to another. It has now been proved by the experiments of Sumner (1932) and others that the racial characters of animals are in general inherited.

The environment, nevertheless, may affect the characters of certain organisms to a considerable degree. The characters of many kinds of plants, for example, are known to be affected by the type of habitat in which they are grown. Many of the characters of animals also are affected by the conditions of their environments. A shortage of food during the growing period, for example, will in most kinds of animals result in their being stunted. Other effects may also be produced, for example, the increase in skin pigmentation in man produced by exposure to intense sunlight. Our knowledge about the degree to which environmental factors affect the characters of most animals is, unfortunately, still meager; many more experimental studies along this line are needed.

Not only may the environment affect the characters of individual animals, but certain factors of the habitat may by reason of their severity entirely preclude the existence of a particular species in a given climatic region. Thus, a species that is unable to endure freezing will be prevented from living permanently in a region where the temperature at any time during the year falls below 32° F. A strain of domestic animal that may thrive in one climate may be unable to exist in a different climate. Certain species of animals are by nature restricted to narrow limits of climate, while other species are more adaptable.

Knowledge of actual limits of tolerance for various climatic factors among the species and races of animals is still lamentably incomplete. Individuals captured in nature and brought directly into the laboratory are often so greatly affected by their captivity that they do not give normal responses. The individuals of many species, furthermore, are able to adjust their physiology so as to adapt to a wide range of environmental extremes. To add to our difficulties, the ability of a given individual to tolerate extremes of particular environmental factors often changes with age, with life-history stage, and for other reasons. Moreover, the heredity of wild-caught individuals is usually wholly unknown, and it cannot be assumed that any particular captive individual actually represents the heredity of any large proportion of its species. Laboratory experiments are consequently subject to many limitations, but they do give much valuable information which aids in the interpretation of field observations of animal distribution.

Laboratory studies of genetics are highly desirable. If we can assume that most of the differences between species and between races have a hereditary basis—and for this we have convincing evidence—then the discovery of the exact number of hereditary factors involved and of the manner in which they produce their effects becomes of great concern to the systematist. Studies of the heredity of specific and racial characters are practical in the laboratory only under very carefully controlled conditions. Such studies are possible only with those species that are able to adapt themselves to captivity. Many species, especially among the higher vertebrates, cannot be success-

fully reared in the laboratory. The chipmunks and squirrels, for example, have not yet been induced to breed well in captivity, and it is consequently impractical to study their heredity. On the other hand, numerous other kinds of rodents have proved very satisfactory subjects for laboratory study.

The precise difference in heredity between any two races or species of animals has not as yet been determined. It is true that we have discovered that certain races and species differ in certain hereditary characters, and we also know that the populations of certain races may differ in the frequencies of certain genes. But in no instance known to me are we able to diagnose any race or species in terms of genes or gene frequencies. When that can be done for at least a few races, and perhaps also for a few species, we shall be in a better position to estimate the possible rate of evolution of systematic categories.

Still another type of laboratory study which is of interest to systematic zoologists is concerned with the growth patterns of species. The various parts of the body of an individual often grow at different rates, so that the animals may present very diverse characters at successive ages. Sometimes, indeed, systematists have been misled into describing several growth stages of the same form as distinct species. Studies of growth may, therefore, simplify the task of the systematist. Such work may also contribute to our understanding of the whole process of individual development from the simple embryo to the highly complex mature organism.

The last type of experimental study to be mentioned here is concerned with the adaptive value of racial and specific characters. Opinions have fluctuated among zoologists about the significance of adaptation in nature, and many violent arguments have arisen. The most convincing evidence on the adaptive value of any particular character will be provided by carefully planned experiments. Unfortunately, the amount of evidence now available from reliable experiments is scanty. Nevertheless, there is good laboratory evidence that at least in certain species of animals natural selection can be a powerful agency in the evolution of adaptation.

Up to this point we have been discussing the collection of animals in the field, the quantitative study of preserved specimens in the museum, and the study of living animals in the laboratory. Let us now turn our attention to certain studies of living animals in nature which may also aid in the solution of problems in systematics.

Field Studies of Habit and Habitat

Field naturalists are well aware that the individuals of any given species rarely are distributed evenly over any large area, but, on the contrary, nearly always occur more commonly in certain situations than in others. Often, indeed, a species will be restricted to a particular type of habitat which occurs only in widely separated small patches. In order to secure specimens of such a species the collector must first learn the habitat preferences of the species and then search for it in such patches of its preferred habitat as he can discover.

These habitat restrictions of animals often provide a clue to the distinctions between races and species, and they frequently suggest that certain diagnostic characters may have adaptive value. Such restrictions to specialized habitats also will sometimes explain why two or more related forms can exist in the same region without coming into disastrous competition with one another. It is heartening to note that systematic zoologists are coming to give increasing consideration to the habitats and habits of the animals that they study.

One result of the habitat restrictions of most kinds of animals is that the population of each species is broken up into many relatively small subpopulations,

which are more or less isolated from each other. The calculations of Sewall Wright indicate that the sizes of these subpopulations and the degree of their isolation from one another have a great deal to do with the speed with which taxonomic differentiation occurs and with the resulting amount of differentiation within the species concerned. Consequently, it becomes of great importance for the systematist to secure dependable information about the organization in the field of the populations of those species in which he is interested. Unfortunately, we as yet know very little about the numbers of individuals which compose the various subpopulations of any natural species of animal, nor do we have much information about the degree to which adjacent subpopulations interbreed. Here is a most important line of study which should receive serious attention from every person who is engaged in zoological field studies.

In recent years zoologists have been able to devise some fairly adequate methods for estimating the population densities of animals living in nature. Improvement in the methods of detecting and of counting individual animals is still desirable, especially for some of the more secretive forms. Nevertheless, it is possible to secure at least a rough estimate of the density of nearly any kind of population and to calculate the total subpopulation that occupies a given type of habitat.

No population, of course, is ever static. Each is constantly fluctuating in numbers through natural reproduction, mortality, and migration. Any complete account of the organization of the local populations of a species must consider these fluctuations in the subpopulations. The consequent necessity of making counts in each of the seasons and in successive years greatly increases the labor of field studies.

In the measurement of the degree of isolation of one subpopulation from another, only slight progress has yet been made. Some of the best information here comes as a result of the banding of birds and their subsequent recovery. It has been demonstrated that most migratory birds return to breed in the general region in which they were born, but not usually in their precise birthplace. With only a few exceptions, however, the records have not been organized to show the amount of isolation, or conversely the amount of inbreeding, between adjacent populations. Complete physical barriers to interbreeding are usually not hard to detect, but ecological and psychological barriers are much more difficult to discover, and as yet we have not been able to devise any very satisfactory measures of their effectiveness.

Another factor that may influence the formation of local races and species is the social behavior and other habits of the individuals. Those species in which the individuals live in compact social groups evidently have a type of population organization different from that to be found in species in which the individuals exhibit strong territorial behavior in the breeding season or which are always solitary in habit. Each restricted social group may be presumed to form a special local unit of evolution and differentiation, while a species in which the individuals are intolerant of one another would be expected to have less well-marked local populations.

Among other ecologic factors that may influence the process of taxonomic differentiation in particular groups of animals, I would mention here particularly (1) the size of home range over which an individual roams as compared to his average distance of dispersal, (2) the rate of reproduction and its fluctuations in response to environmental factors, (3) the presence or absence of seasonal migration, and (4) those selective factors in the environment that may direct the course of evolution. The systematist may well find possible explanations for the observed differentiation into races and species by studying the ecology of the animals involved.

Practically all systematic zoologists are interested not only in classifying the races and species of animals but also in the evolutionary history of their group and in those factors that have been and are still directing its course of organic evolution. Systematic zoology is consequently concerned with most of the fundamental problems of biology, including especially the control of body form, growth pattern, ecologic response, adaptation, regulatory mechanisms, animal behavior, and mechanism of heredity. In its turn, systematic zoology can be of assistance to workers in almost every other branch of biology. It is very gratifying to note the steadily increasing amount of cooperation between systematists and other biologists. Systematists can greatly profit by using the quantitative and experimental methods which have been developed in other branches of biology, just as students in these other branches of biology need to use the conclusions reached by systematists.

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U.S.C. MARINE LABORATORY
VELERO IV

Captain Allan Hancock, First Patron of the Society of Systematic Zoology

CAPTAIN ALLAN HANCOCK, who with this announcement becomes the Society's first patron, has had a long and notable association with scientific exploration and research. Although his interest in science has extended to all phases of oceanography, to paleontology, to agriculture, and to many other fields, it has been especially fruitful in zoological exploration and research in systematics. His very substantial participation in this field and continued support of the work of taxonomists make it very appropriate for him to be a patron of the Society of Systematic Zoology and for the Society to honor him for his achievements.

As a boy Allan Hancock mined the asphalt deposit on the family homesite, Rancho La Brea, finding there the Pleistocene fossils which were Los Angeles' first claim to scientific distinction, in the opinion of such discerning visitors of the period as Col. Theodore Roosevelt and Thomas A. Edison. More recently, since 1931, as master of two ocean-going vessels, *Velero III* and *Velero IV*, Captain Hancock has led a series of expeditions to tropical America with attention centering on the Galapagos Islands, the Gulf of California, and the coast of Peru. North American institutions which have benefited from participation in these expeditions include the U. S. National Museum, the California Academy of Sciences, the San Diego Zoological Society, the Museum of Comparative Zoology, and the Universities of Nebraska, Michigan, Stanford, and British Columbia.

Following the gift of the *Velero III* to the University of Southern California in 1939, Captain Hancock erected on this metropolitan Los Angeles campus the Allan Hancock Foundation, a spacious building designed to serve as a repository for collections obtained on the numerous voyages and as a center for research in marine biology, oceanography, telecommunications, and aeronautics. The Foundation, which has a scientific and technical staff of 32 persons, also houses the Hancock Library of Biology and Oceanography and both radio and television broadcasting studios. In addition, research space is provided for several university departments, notably biochemistry and experimental medicine.

Not the least of the Foundation's accomplishments under the far-sighted direction of Captain Hancock has been the publication of a series of monographs by competent systematists on various plant and animal groups: on marine algae by Wm. R. Taylor, on hydroids by C. M. Fraser, on bryozoans by R. C. Osborn, on crustaceans by W. L. Schmitt and L. B. Holthuis, and on echinoderms by H. L. Clark, as well as others prepared by members of its own staff. Captain Hancock has thus served as benefactor to biological science in three different capacities, by leading the explorations which collected the specimens, by providing suitable accommodations for the assemblage and study of this material in an environment conducive to productive research, and by arranging for the dissemination of results through the printed page.

The Society is proud to welcome as patron a man of Captain Hancock's distinction.

Captain Allan Hancock

The Nature of Systematic Biology and of a Species Description

GEORGE S. MYERS

SYSTEMATIC biology has broadened immensely in recent years, so much so that it is difficult for any one person to keep familiar with its ever-widening boundaries. Every worker in this field, whether he knows it or not, has become a more or less narrow specialist practicing only certain types of systematics, quite aside from whatever group-specialty he may profess. As a result, almost every recent attempt I have seen to define or describe the field as a whole omits great segments of the science—segments which are usually recognized by the workers in them as part of systematics.

A few systematic plodders are still willing to stultify both themselves and their field by asserting that systematics exists solely to classify and name specimens for the benefit of other and, it may be inferred, more truly scientific lines of endeavor. Most assuredly such persons have not kept up with even the most widely advertised progress in their own field. If systematics is a part of biology at all, and few will deny that it is, its devotees must realize that it does make real contributions to scientific thought from and of itself. It is not a mere technological service.

What is Systematics?

Let us take a variety of sample studies and ask ourselves if these are truly a part of systematic biology, to wit: (1) a phylogenetic study of fossil horses; (2) an examination of brain differences between the species of a genus of fishes; (3) an experimental study of genetic (and phenotypic) differences between two species or subspecies of fruit-flies; (4) an evaluation of the importance of life-history data in working out the relationships of amphibian families; (5) a study of the effect of seasonal population movements upon the differences between two intergrading races of field mice; (6) chromosomal structure in the species of a genus of earwigs; (7) differences in the psychological reactions of two closely related species of monkeys; and (8) the serological relationships of the antelopes.

Are studies such as these systematic biology? Some backward systematists would say they are not, but the thoughtful modern worker would, I think, find it difficult to exclude from systematics all or the greater part of any one of the eight problems listed. Each of these problems contributes principally or wholly to a knowledge of the nature or origin of the natural populations of organisms, and I do not see that we can limit systematics any more closely than this.

I therefore submit my own definition of systematic biology, a definition which, in its essentials, I have been teaching to my students for the past twenty years: *Systematic biology (="systematics") is the study of the nature and origin of the natural populations of living organisms, both present and past.*

It will be noted that the word "nature" is of very broad intent, and that it comprehends, in this instance, all of the varied researches which make up the modern systematic field. Indeed, even the words "and origin" are clearly redundant, but I prefer to include them merely to emphasize the essentially systematic nature of studies of evolution and genetic relationships between populations. Species are populations of a certain grade. So are subspecies and demes. And species

make up the higher and more subjective populations (past and present) that we call genera, families, and higher groups. As to "natural populations" and its meaning, I leave the question open, for consideration of the "unnatural populations" of highly modified domestic animals soon leads into fields that have traditionally been, and are still generally considered to be, a bit beyond the true limits of systematics; logically, however, they should be included.

The principal objections I have heard to the above definition concern its broadness. A large part of biology is included. But insofar as any biological study contributes definitely and consciously to an understanding of the nature of organisms *as populations,* I cannot see how we can deny that it is essentially systematics. Population dynamics, so called, must be included just as surely as population genetics, simply because both form basic parts of our knowledge of species, subspecies, and other populations. Moreover, ecology and biogeography are included, but only insofar as they contribute to knowledge of the populations themselves. Ecological preferences, bird songs, and even psychological differences may be just as "good" specific or generic characters as are morphological criteria.

The dynamic aspects of this definition are important. If the systematist views his work in this broad way, he will soon realize that the examination of dead samples is only a small part of his science. What he is dealing with is not a set of dead specimens, but living, changing populations out of doors. I believe this to be the most important concept the modern systematist has to learn.

If we accept this broad definition of systematics, there are several philosophical implications which may not be immediately apparent. If systematics deals primarily with the nature of populations, such appurtenances as nomenclature are seen in proper perspective, as mere adjuncts to systematics, necessary in speaking of populations but of minor importance. Even the age-old question "What is a species?" fades into relative obscurity as only one of a whole series of parallel questions. What is a subspecies? What is a deme? These questions are just as important, but all of them are, in the last analysis, quite as nomenclatural (or semantic!) as biological. "Knowledge of the populations themselves is everything, and the nomenclature relatively immaterial"; this is the advice I give my graduate students in systematics.

Another point to which systematists have given insufficient thought concerns the whole subject of scientific research. If, as most scientific workers seem to agree, research *is addition to knowledge,* through discovery or re-interpretation, some things the systematist does are scientific research and some are not. Work that adds to knowledge of populations would then be scientific research, but the preparation of keys and manuals compiled solely from already known facts would not. Rather, would they be a by-product of research, albeit an extremely useful and important by-product. Most such keys and manuals, if done by more than a hack, do contribute some new and essential data to an understanding of species and other population, and, insofar as they do, their preparation is unquestionably research in the science of systematic biology. However, it is difficult to call the ordinary routine identification of specimens by non-specialists either scientific research or systematic biology; it is mostly routine technological use of scientific results.

Classification

What, it may be asked, has happened to "classification," which has formed a part of almost every definition of systematics? It will repay us to discuss this briefly. Despite the criticisms of those who have attempted to throw doubt upon the phylogenetic ideal of biological classification,[1]

[1] See especially W. R. Thompson, 1952. The philosophical foundations of systematics, *Canadian Entomologist, 84,* 1–16.

the most important, and well-nigh the only, guiding principle of modern classification has resided in the common effort to express, by means of this classification, the phylogenetic relationships of populations, insofar as these relations are demonstrable or inferred by a process of evolutionary extrapolation. That few, if indeed any, such relationships are susceptible to absolute proof (in the legal sense), and that no man-made hierarchical system of categories is capable of expressing precisely all the observed or inferred facts, are poor reasons for deserting the phylogenetic ideal in such classification. For if we do desert it, systematics inevitably returns to its pre-Darwinian status as a method of merely pigeonholing information. Mere morphological or other similarity, if divorced from evolutionary ideas, would permit the building of classifications that would mean little from almost any point of view, except that of pure practicality. I greatly doubt that any worker who has had the privilege (relatively rare in some modern systematic fields) of carefully working out, de novo, the "phylogenetic" classification of any sizeable animal group, even in the absence of fossil evidence, will agree with anyone who maintains that his findings do not demonstrate a large body of facts bearing upon the phylogeny of the included populations.

It should, of course, be unnecessary to point out that phylogeny is the past history of populations, that the word "history" is of broad meaning, and that the past includes not only paleontological history but also what happened only a second ago. Yet some of those who reject phylogeny as the prime guiding principle of classification appear to have forgotten these concepts. In other words, phylogeny might well be defined as the history of the nature of organic populations; as such, it is inseparable from studies based wholly upon living populations.

Classification is, in my view, primarily a method of expression of what we know of the nature and relationships of living populations. As it deals with entities which, on the whole, are discrete and have diverged from each other, it is largely an expression of that phylogenetic divergence. To deny that the divergence is of an evolutionary nature because of lack of absolute proof is to deny evolution. To deny that classification has a phylogenetic basis for the same reason appears to me as quibbling. And if classification is chiefly *a method of expression* of our present knowledge of the nature of organic populations (remembering always that this knowledge is incomplete), there is no particular reason to mention it in a definition of systematics, any more than we should necessarily mention Arabic numerals in a definition of mathematics.

The Scientific Method in Systematics

One of the ways in which systematists have tended to differ profoundly from other scientists is in their frequent disregard of any recognizable application of what is commonly termed the scientific method. It is generally understood that a part of this method is the *statement of the problem.* Of this, I believe, one of our greatest physicists has said that a problem properly stated is often a problem partly solved. The average working systematist seldom thinks of a problem in this way, except when he has difficulty in revising some groups of species or genera, and then only in a very limited sense. Usually he simply "does some work on" a group, and forthwith publishes some possibly ill-thought-out results. Some have insisted that ordinary systematic work, done with dead-specimen samples, does not lend itself to problem-statement, or results in such an infinity of problems that formal statement of them becomes absurd. In a way this is true, but the complete absence of any verbal formulation of what the worker is trying to do usually results in a type of mental anarchy to which most systematists have been especially prone.

One of the hoped-for consequences of adoption of a broader definition of system-

atics is the aid it may give the systematist both in formulating and stating definite problems and, through this mental discipline, in enabling him to perceive shortcomings in even the lesser of his systematic efforts.

To some systematists it may seem magnificently absurd to try to formulate a statement of the problem when beginning a systematic revision of the species and genera of any group. Yet, if the worker states that he is attempting to redefine the limits of genus *X-us* and of its species, to redefine their ranges both geographically and ecologically, and re-evaluate the phylogenetic relationships, and *keeps this statement before him as he works,* he is not likely to omit, as I have seen competent systematists do, parts of the study that are obviously essential in solving the problem. Moreover, the way in which the systematist presents his data is likely to be far more logical, and to answer more clearly the essential questions, than would have been true if he had merely begun "to revise the genus" without a statement of objectives. The problem should be stated in the introduction to the paper; it then becomes logical, at the end, to append a set of the more important results or conclusions.

The effects of such a regime may not at first be apparent, but if every systematist were required to make a statement of his problem at the beginning of each of his papers, and to state the principal conclusions at the end, a great many writers would not be especially proud to acknowledge authorship of certain papers or to publish others like them in the future. For if no reasonable statement of the problem can be made, the resulting paper is probably not worth publishing. Moreover, one beneficial consequence would be that non-systematists would at last begin to realize, as many still do not, that systematics is an adult scientific field, with definite problems and results, and not merely a vast set of pigeonholes into which the systematist endlessly pops specimens.

Nature of a Species Description

It is interesting to apply these ideas to isolated descriptions of supposed new species. How may one formulate a statement of the problem in such papers? It has long been apparent to thoughtful systematists that the average "description of a species" is nothing of the sort. Almost invariably it is merely a description of the specimen samples available to the author, and frequently of not all of these. It certainly never contains an account of more than a small fraction of the many thousands of characteristics of the species. Philosophically, then, what is this common type of description? Some twenty or more years ago it occurred to me that the ordinary species description (whether of a "new species" or an old one) is nothing more nor less than a prediction—*a prediction that a population, agreeing more or less well with the descriptive information given, exists or existed at the place and time whence the specimens came, and that it is or was of the grade commonly designated by the word species.* A great deal of future work may be required to prove the truth or falsity of the prediction. If the prediction proves to be true, the species is generally recognized. If not, the prediction falls into obscurity, and the proposed name falls into the synonymy.

From this point of view, there is no great difficulty in formulating a statement of the problem for announcing or predicting a new species or other population grade. Does or does not the specimen-sample in hand indicate the existence, at some specific time and in some specific area, of a hitherto unrecorded population of specific (or subspecific) rank? I suspect that most systematists would hesitate to print such a statement in connection with every "new species" they announce, simply because it seems so obvious what they are doing. Nevertheless I believe that merely holding in mind this problem statement would improve both the thinking and the published work of the systematist.

Addendum

The above paper was submitted in August 1951, but it was not until February 1952 that Dr. Blackwelder kindly sent me galley proof of the paper by Dr. Boyden and himself treating much the same subject (SYSTEMATIC ZOOLOGY, *1*, 26-33, 1952). While my paper was in no sense intended as a comprehensive treatise on the nature of systematics, I find myself in disagreement with Blackwelder and Boyden on many points, and wish to make some supplementary remarks.

In their classification of the zoological sciences the three main divisions appear to me partly superficial, and would certainly be so considered by a majority of working biologists. A much more fundamental, as well as a far more widely recognized and utilized, classification would begin with the segregation of those sciences dealing with what might be called the internal dynamics of organisms (physiology, both organ and cellular; genetics, both classical and physiological; experimental and chemical embryology; biochemistry; pathology; much of psychology; etc.) from those which deal principally with populations of organisms (systematics; ecology; zoogeography; population dynamics; etc.). In an intermediate group, and rapidly being absorbed by the first or second groups, or both, would be placed such sciences as the "comparatives" (anatomy, physiology, cytology, embryology, etc.). The portions of these sciences which deal with the human species are now generally included under medicine, anthropology or sociology. That Blackwelder and Boyden can see a fundamental philosophical difference between the general purposes of, for example, genetics and physiology suggests an insufficient evaluation of modern trends in these almost merging fields. Similar comment could be made on other aspects of their subdivision of zoology. The strange way in which they regard several of the zoological sciences has undoubtedly had its effect on their view of systematics, but I will take no further space for delving deeper into this subject.

While I agree with Blackwelder and Boyden that neither the nature nor real aims of systematics are correctly understood, I flatly reject a good many of their ideas on the subject and feel that there is a large and growing body of modern systematists who will also reject many if not most of the same ideas. Specifically, I reject their idea that the phylogenetic concept has been a misorientation of, and hindrance to, systematics; rather it has been a guiding light too often forgotten. I submit that a far more debilitating effect upon systematics has been achieved by the still prevalent idea that systematics exists solely as a practical handmaiden to other sciences. Specifically, and with knowledge of Dr. Boyden's special views, I reject the idea that the phylogenetic concept in homology has been as widely misused or misunderstood as some would believe.

Also I cannot understand why Blackwelder and Boyden appear to confuse Dobzhansky's and Mayr's *species definitions* with general concepts of the nature of systematics, or just what that passage in their paper is intended to demonstrate. And, specifically, I should have liked to see the authors define what they mean by "essential nature," when they propose to use this as the substitute for a phylogenetic basis in systematics.

My view is that Blackwelder and Boyden's "aims and tasks of the taxonomist" (numbered one to seven) are only in part essential aims, and that the first point, regarding the collection of specimens, is pure methodology and no more needs inclusion in a general statement of the content of systematics (or taxonomy) than does the catching of a dog or an amoeba on which to experiment need inclusion in a statement of the content of physiology. Number two, regarding the description of the collected specimens, is again a methodological way of getting at certain types of systematic (or taxonomic) information, but is not invariably essential to sys-

tematics as a science. Number three, which concerns the comparison of specimens, is so generally understood that it could be covered, with some of the other points, in a more general statement. Numbers four and five, about developing classificatory principles, and placement in categories, are certainly essential. Number six, dealing with keys and other devices, I regard as clearly involving by-products of systematic research, and number seven seems merely a piece of fatherly advice. Frankly, most of these "aims and tasks" do not seem aims at all, but rather tasks (or methods) by which the systematist often goes about his research.

Their "aims and tasks of the phylogenist" (in five points) on the other hand, appear to me to be things to which *every* systematist, not only the phylogenist, should subscribe, even though they might be stated very differently. Specifically, points Numbers one and two, with regard to genetic mechanisms, although quite different, might easily have been merged into one statement which would and should be more inclusive of genetic factors than are the two points together. Moreover, if these statements are included, why should not there be something about the environment and natural selection as well as intracellular genetic mechanisms? These things also are important in understanding evolution.

Although Blackwelder and Boyden believe that the listing of these "aims and tasks" of taxonomist and phylogenist should help clarify the relations between them, I am not at all sure that they have. Nor do I believe that they have made their chief point that the phylogenetic ideal of systematics "is not now and never has been justified." While I realize full well that in few or no instances are there the actual specimen-samples to prove phylogenetic relationship, the stubborn fact remains that most forward-looking systematists of modern times have attempted to bring classification into the closest possible accord with whatever evidence of phylogenetic relationship exists. This is often done in the face of facts which, in the absence of the phylogenetic concept, might have dictated a different arrangement. That a perfect phylogenetic system is an ideal never quite attainable affects the usefulness of the concept not one whit.

Finally, inasmuch as Blackwelder and Boyden admit that, in general, their "present-nature" and the phylogenetic method will, if correctly applied, yield concordant results, I utterly fail to see wherein the phylogenetic method could have had quite so stultifying an effect on systematics as they seem to believe! As for myself, I intend to continue to consider the phylogenetic insight which systematics provides as the principal goal of most systematic work, as the principal guiding idea in systematics, and as the principal reason why modern systematic research has become a respectable modern science in its own right.

GEORGE S. MYERS, Professor of Biology and Curator of Zoological Collections, Stanford University.

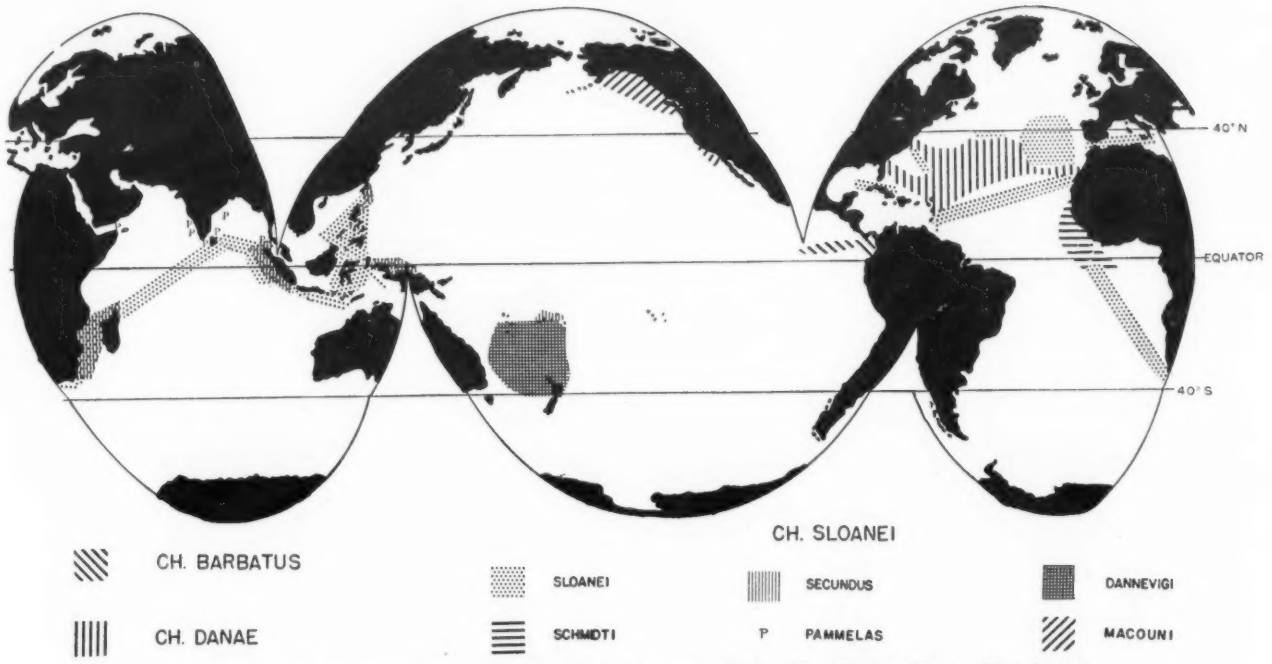


FIG. 1. Horizontal distribution of the species and subspecies of *Chauliodus*. Map based on Goode's Homolosine Equal-Area Projection, Copyright by the University of Chicago. Used by permission of the Univ. of Chicago Press.

Zoogeography of the Bathypelagic Fish, *Chauliodus*

RUDOLPH E. HAFFNER

THE MID-DEPTHS of the oceans comprise a significant part of the volume of the biosphere, yet the conditions for life here are so different from those encountered by man that it is difficult to appreciate the factors limiting the distribution of the bathypelagic organisms living there. Catch records, however, indicate that many species are restricted in their geographical distribution. An attempt is made here to study the conditions of life in these mid-depths by centering attention on a group of fish that has a world-wide distribution. There are a number of fish groups which could serve for such a study, among them the gonostomiatids and the myctophids. Although these two groups have a wide distribution and occur in large numbers, their taxonomy has not been sufficiently clarified to make a zoogeographic study possible. The genus *Chauliodus* was chosen because it fulfilled the requirements. It has a world-wide distribution; large numbers of body measurements are available for the comparison of different populations; physical and chemical data are available for the stations at which the specimens were caught.

The Stomiatoidei, the suborder to which *Chauliodus* belongs, can be considered herring-like fish with photophores and extremely large teeth. The genus *Chauliodus* has been placed in the family Chauliodontidae by Berg (1940), removing it from the Stomiatidae because the inner ear of *Chauliodus* lacks a lagena, whereas all the other members of that family have one. These two families make up the superfamily Stomiatoidae in the suborder Stomiatoidei. This suborder of the Clupeiformes is characterized by Berg as being "near the Clupeoidei, especially near the Alepocephalidae." (p. 431).

Although *Chauliodus* (Plate 1, p. 120) lacks scales, the pattern of the pigment is such that it forms hexagonal areas in which there is a thin deposit of an opalescent substance, which appears under magnification like guanine crystals. The body is covered by a thick transparent coating containing microscopic spheres of unknown composition. This gelatinous layer forms, in part, the base of the adipose fin, and does not seem to be merely a slime secretion. Brauer (1908) was of the opinion that the gelatinous layer is a part of the corneum of the skin, although his earlier observations had led him to believe that it was a slime secretion. The fang-like teeth (Plate 2) appear admirably suited to the capture of the animals found in the mid-depths, and, indeed, stomach analyses show that *Chauliodus* feeds on chaetognaths (Clemens and Wilby, 1946) and on macroplanktonic crustacea, probably deep-sea prawns (personal observation). It is tempting to speculate that the prey is lured to the mouth by the many lights of the photophores. These light organs are of three types: microscopic spheres without a pigment layer; large spheres with a pigment coat, reflector and lens; and larger, bell-shaped organs with a pigment coat, lens and reflector. The microscopic photophores are scattered over the dorsal surface of the fish, whereas the larger, pigment-covered spherical organs are found in groups within each hexagonal "scale area," around the eye socket, and in rows along the dorsal surface of the base of the pectoral and pelvic fins, and the base of the dorsal and anal fins. There is also a lateral row between the rows of bell-shaped organs. The two suborbital photophores (Plate 2) are special types, the more an-

terior being double, composed of two spheres joined together, the other, although it appears as an elongated, spherical organ, has certain microscopic differences which distinguish it from the other spherical photophores. The bell-shaped organs, the most conspicuous photophores, are arranged in four rows, one lateral row on each side and two ventral rows. The photophores which line the inside of the lower jaw are also of this type. In addition to those described above, there are light organs on the minute barbel (Plate 2), and on the tip of the very elongate first dorsal ray, which is hinged and provided with muscles so that it can be directed forward.

Ege (1948) has revised the genus using proportional measurements, dentition, photophores, fin rays and other meristic characters, as taxonomic criteria. According to his revision, the genus *Chauliodus* has three species: *Ch. danae* Regan and Trewavas (1929), *Ch. barbatus* Garman (1899), and *Ch. sloanei* Schneider (1801), the last being represented by the subspecies *s. sloanei, s. pammelas, s. dannevigi, s. macouni, s. secundus* and *s. schmidti.* Ege's subspecies *s. pammelas* was originally described by Alcock in 1892 as a species. His subspecies *s. dannevigi* was originally designated as a species by McCulloch in 1916, and *s. macouni* was described as a species by Bean in 1890. Ege's classification will be adhered to in this paper, although his designation of subspecific rank for some of these groups is questionable in the light of modern taxonomic procedure. The necessary taxonomic changes will be the subject of a subsequent paper (Haffner, in preparation).

Although Ege (1948) includes the *Dana* station number, haul number, and meters of wire out for the various specimens which he examined, he made no effort to correlate this information with the hydrographic data obtained by the *Dana* Expedition (*Dana* Reports 1, 12, and 26), or any other. A large part of this present paper is concerned with a correlation of all available taxonomic and ecological data.

The Chauliodontidae are distributed around the world in a belt between 60° N. and 40° S. with the major part of the group concentrated between the 40° parallels (Fig. 1). The exceptions are the large population of *Ch. s. macouni* in the Gulf of Alaska, and a few specimens of *Ch. s. sloanei* and *Ch. danae* taken in the North Atlantic between Newfoundland and Europe.

The Atlantic Ocean has three groups: *Ch. danae* in the North Atlantic, mostly concentrated in the western part, *Ch. sloanei sloanei* in the North and South Atlantic with a sharp discontinuity between the two populations and *sloanei schmidti* in the area separating the North and South Atlantic populations of *s. sloanei.*

Three subspecies of *sloanei* are found in the Indian Ocean. Along the east coast of Africa there are *sloanei,* continuous around the tip of Africa with the South Atlantic population, and *secundus.* The subspecies *pammelas* around the tip of India, occurs in part, together, with *sloanei.* Only *sloanei* is found east of Ceylon until one reaches the waters west of Sumatra where again it is found together with *secundus.* The only subspecies in the Indo-Malayan Archipelago is *sloanei.*

Four subspecies of *sloanei* are found in the Pacific Ocean. In the western part, around the northern end of the Philippines and north of New Guinea *sloanei* and *secundus* occur together. Off the Samoan Islands *secundus* again occurs, overlapping somewhat with *dannevigi.* (This is unfortunately not indicated on the distribution map, Fig. 1). The *dannevigi* population extends between Samoa, Australia and New Zealand. The genus is represented in the eastern part of the Pacific only by *sloanei macouni* which is limited to the Gulf of Alaska and the waters off the coast of California. *Ch. barbatus* is found in the Gulf of Panama and in the waters off the Galapagos Islands.

In general it may be stated that each species or subspecies inhabits some particular part of the ocean and that there is relatively little overlap. Important exceptions are the overlap of *s. sloanei* and *s. secundus* in the Indian and Pacific Oceans, and the Samoan Islands populations of *s. secundus* and *s. dannevigi.* The two North Atlantic species *danae* and *sloanei* show slight differences in concentration of their populations; *danae* is more frequent in the western part of the Atlantic, *s. sloanei* in the southern and eastern parts of the Atlantic. The latter is the only representative found in the Mediterranean Sea. It is interesting to note that tows which brought up both species were usually made in the vicinity of small island groups. Both species have been taken together around the West Indies, Bermuda, the Cape Verde Islands, the Azores, and the Canary Islands.

Vertical Distribution and Migration

It is necessary to consider the vertical distribution of the Chauliodontidae in addition to their horizontal distribution. This consideration is particularly important if there is overlap in the horizontal distribution, for although the areas of two populations when drawn on a map may have the same geographic boundaries, the populations may, in fact, be separated vertically.

The vertical distribution of each population was graphed by plotting the different sizes of individuals as a percent of the total catch within each depth interval against the depth (calculated as one-half the length of the cable) at which they were caught. The actual number within each interval was also indicated. The day and night hauls were plotted separately. The few hauls that were made during the twilight hours were somewhat arbitrarily placed into either day or night hauls. All hauls from 0530 hours to 1830 hours were considered daylight hauls. Such graphs (Figs. 2, 3) show a consistent and remarkable difference between the vertical position of a population by day and by night. The most reasonable explanation of this is that the fish undergo an extensive vertical diurnal migration.

The earlier *Dana* expeditions made tows day and night at all depths and thus obtained samples that were useful for determining diurnal migrations. It soon become apparent to the collectors, however, that night hauls would produce more specimens of bathypelagic forms at considerably reduced wire lengths, and the program became geared to night tows with shorter wire lengths. Deep tows made at night and shallow tows made during the day produced relatively little in the way of bathypelagic fauna. It would appear from this observation that most of these forms come to the surface at night and go deeper in the daytime. Repeated attempts at various stations during the 1928-1930 world cruise of the *Dana* substantiated these findings. This procedure, i.e., night hauls made in shallow depths, day hauls made only at great depths, alone would not yield sufficient information about the vertical distribution of the fish. However, the fact that all of the relatively fewer tows made at great depths at night yielded no *Chauliodus* is sufficient indication that very few remain at daytime depths during the night. Also, not catching *Chauliodus* in shallow daytime hauls is reasonably explained only by their absence. The familiar argument of net avoidance in the upper water layers during the day has received undue emphasis in the past to explain poor catches. It is reasonable to assume that the bathypelagic organisms are no better swimmers than many of the surface pelagic fish, and these are successfully caught during the day in upper water layers with the same type of gear.

Although vertical diurnal migration stands as the most reasonable explanation of the difference between daytime and nighttime distribution, more direct support of this hypothesis was sought. The deep scattering layer (D.S.L.) can be observed on a fathometer tracing to un-

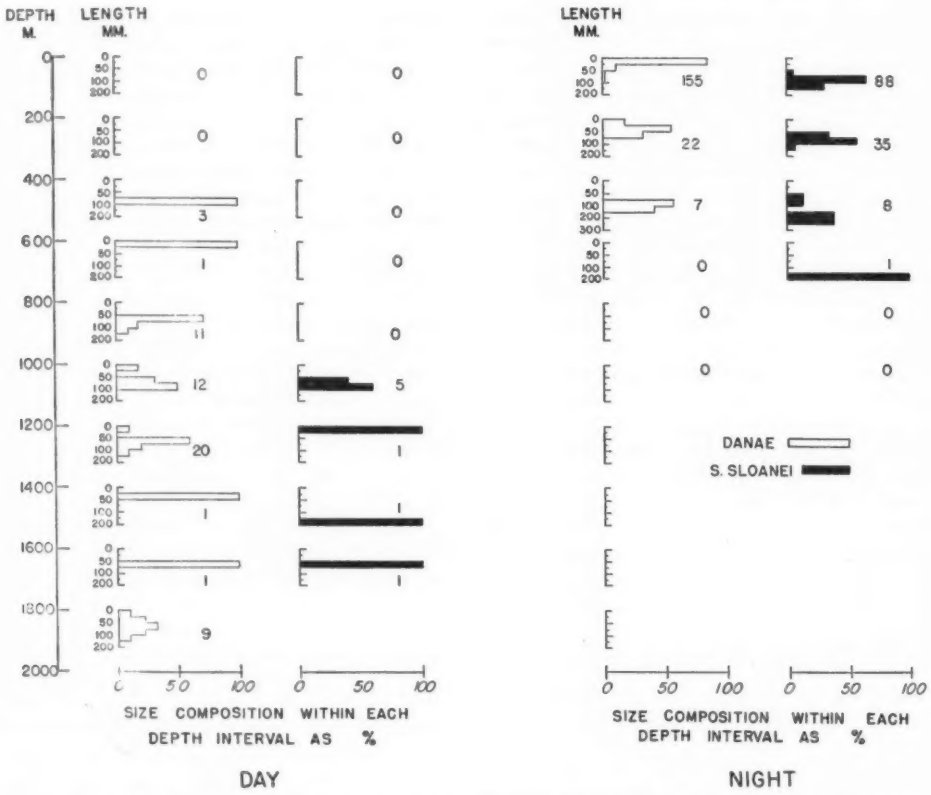


FIG. 2. The vertical distribution of *Ch. danae* and *Ch. sloanei sloanei* in the North Atlantic by day and by night. The histograms indicate the size composition of the catches within each 200-meter depth interval. Number beside each histogram gives total catch for that depth interval.

dergo vertical diurnal movement (Dietz, 1948). This sound scattering layer starts toward the surface at sunset and descends to the mid depths at sunrise. If it could be demonstrated that *Chauliodus* is associated with the D.S.L. and in particular with that part which is known to migrate, then the existence of its vertical migration is beyond doubt.

Tucker (1951) has been able to show that the Myctophidae and other families of fish (which he unfortunately does not name) are a part of the D.S.L. This was done by means of a closing net equipped with a bathygraph, an instrument recording depth and duration of tow at that depth, towed at the depth of the D.S.L. as determined by a fathometer. Hersy and Moore (1948), and Boden (1950) are of the opinion that euphausiids are mainly responsible for the D.S.L. echo. Others believe that the sound scatterers include squid (Lyman, 1948) and fish with swim bladders (Marshall, 1951).

Hersey, Johnson and Davis (1952) used surface explosions to obtain a wide range of sound frequencies, and by selecting echo frequencies, from 2 kc. to 20 kc., found that the D.S.L. is composed of different sized scatterers, the smaller ones

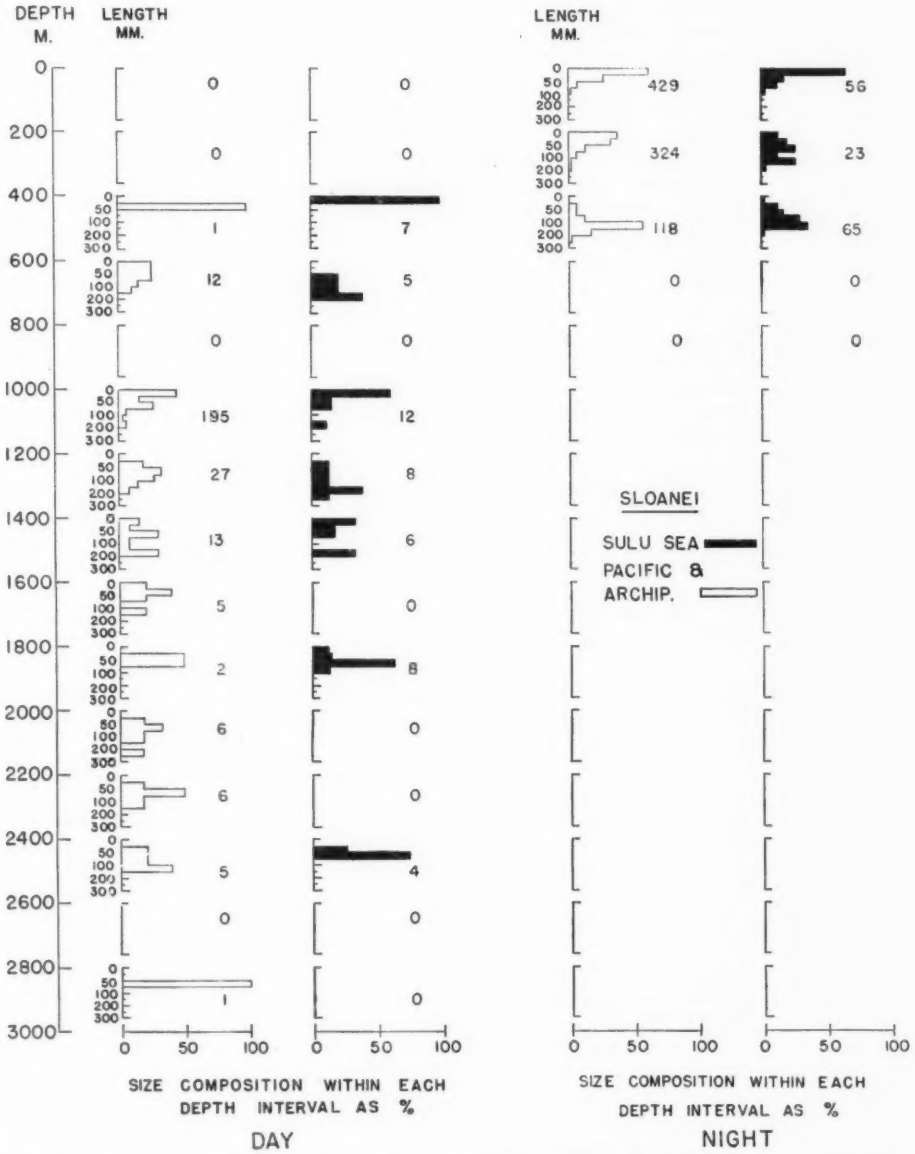


FIG. 3. The vertical distribution of *Ch. s. sloanei* in the Sulu Sea, the rest of the Indo-Malayan Archipelago and the Pacific Ocean. For further explanation of graph see legend to Figure 2.

being near the top of the layer, the larger ones forming its bottom. This distribution refers only to the major concentrations, since it is possible to detect small scatterers all through the layer and some of the large ones are also found near the top.

An attempt was made by the author to determine whether *Chauliodus* belongs to the D.S.L. Several tows were made in the vicinity of Bermuda using the same equipment as described by Hersey, Johnson and Davis (1952) to determine the depth of the D.S.L. Since the net was not equipped with a closing device, it is possible that all of the organisms did not come from the recorded depth. The bulk of the material in the first two hauls consisted of macroplanktonic crustacea and the small gape-mouth fish, *Cyclothone*. The single specimen of *Ch. danae* was 29 mm., the same size as the *Cyclothone*. The size of the organisms suggested, if Hersey, Johnson and Davis are correct, that the net was fishing the upper levels of the scattering layer. Checking the depth of the net with a deep-water bathythermograph showed that indeed the net had not been down as far as had been assumed from the meters of wire out. Subsequent deeper tows produced larger specimens of *Ch. danae* (70-80 mm.) and other stomiatoids. The composition of each catch was about the same, the fish portion consisting in the main of stomiatoids, myctophids, and an occasional deep-sea-angler fish. The invertebrates taken in the tows were primarily chaetognaths, euphausiids, and the larger copepods. However, the mesh size was much too large to take a representative sample of this segment of the D.S.L.

Evaluation of the evidence presented above leads to the conclusion that the *Chauliodus* is a part of the fish population of the D.S.L. during the daytime. Unfortunately, continuous series of simultaneous tows and fathomograms during the actual migration have never been made. However, the evidence of the association of *Chauliodus* with the D.S.L. during the day, together with the results of the analysis of the catch records of the *Dana* expedition make it reasonably certain that these fish are a part of the D.S.L. which does come to the surface at night.

In discussing the vertical distribution of the populations of *Chauliodus*, it is convenient to consider each ocean basin separately and treat all the populations in each. The Atlantic has two species, *danae* and *sloanei*, the latter represented by the subspecies *sloanei* and *schmidti*. The depth distribution in the North Atlantic, seen in Figure 2, indicates that *danae* is taken at considerable depths during the day, from 500 to 2000 meters. At night, the distribution is between 500 meters and the surface. By "surface" is meant the upper 50 meters of water. The graph also shows that the larger specimens are found in the deeper water in the day and do not come as close to the surface at night.

The depth range through which *s. sloanei* is taken is much narrower than that of *danae*. The daytime distribution is between 1000 and 1800 meters, while at night the fish is found in the upper 800 meters of water. The graph (Fig. 2) indicates that the larger specimens tend to be more deeply distributed both by night and by day, being in this respect similar to *danae*.

In the South Atlantic where both *s. sloanei* and *s. schmidti* are found, *schmidti* has a greater depth range than *sloanei*. While *schmidti* ranges from 500 to 3000 meters, during the day, *sloanei* has about the same depth range as the North Atlantic population, 1000 to 1500 meters. The nighttime distribution of both is in the upper 500 meters of water. The same pattern of size categories and depth distribution occurs, the large fish occupy the deeper portions of the vertical range, both during the day and at night.

The Indian Ocean has three subspecies of *sloanei*: *sloanei*, *secundus* and *pammelas*. The vertical distribution of *sloanei* during the day is 750 to 2200 meters, with a gap in the interval 1600-1800 meters,

where tows were made without catching any *Chauliodus*. The nighttime distribution is between 500 meters and the surface, and again the larger specimens are predominantly in the 500 meter depth. Nothing can be said about the daytime distribution of *secundus* since only one such tow yielded any specimens. These four specimens were taken at 1000 meters. It is possible that *secundus* occurs much deeper, and that the trawling operations failed to go deep enough. The night hauls produced 118 specimens taken in the upper 500 meters of water with the largest individuals caught in the 500 meter zone. The even more scanty data available for *pammelas* suggest that the depth at which this subspecies is found may be somewhat deeper than those of other subspecies of the Indian Ocean.

The vertical distribution of *sloanei* of the Indo-Malayan Archipelago is given in Figure 3. The Sulu Sea population has been plotted separately in view of the morphological differences which distinguish it from the rest of the populations in the Archipelago. However, the vertical distribution of the Sulu Sea form shows no marked differences when compared to the rest of the Archipelago and the Pacific regions. With the exception of one specimen taken at 2800 meters in a daytime haul in the Banda Sea (see Fig. 10 for map of archipelago), the daytime distribution of *s. sloanei* throughout the entire archipelago is between 500 and 2500 meters. At night the *Chauliodus* of the whole region are in the upper 500 meters, with the larger fish staying in the deeper part of this zone. In both populations there is a gap between 800 and 1000 meters in the daytime distribution, and in the Sulu Sea population there are two additional gaps; one between 1600 and 1800 meters and another between 2000 and 2400 meters. These gaps are not the result of lack of tows in these depths.

The discussion of the depth distribution of the Pacific populations will be confined to *sloanei secundus, sloanei dannevigi* and *barbatus,* since *s. sloanei* has been included in the discussion of the Indo-Malayan Archipelago, and the data for *s. macouni* are incomplete. All that can be said about the depth distribution of the latter is that its limits are 800 and 150 meters. The species *barbatus* lives between 1000 to 2000 meters during the day, and although hauls were made at night in the upper 500 meters of water no specimens were caught. This does not necessarily mean that *barbatus* does not migrate upward at night. It may only mean that the extent of the movement is much less than that shown by the other groups of the genus. During the day *secundus* has been taken in the Pacific between 200 to 1600 meters, with gaps between 400 and 800 meters and 1200 to 1400 meters. At night the depth range is from 500 meters to the surface, the larger specimens occupying the 400-500 meter layer. The depth distribution of *dannevigi* at night is between the surface and 500 meters. The daytime distribution is between 1000 and 2500 meters. This gap between the night and day distribution is not due to lack of tows at these depths. At *Dana* station 3587, two specimens were taken at 300 meters and one at 150 meters. A total of 13 other tows were made at this station from 1600 hours to 2305 hours at depths from 2500 meters to 50 meters, but no other specimens than the three mentioned were caught. Daytime hauls at station 3593 also produced no specimens in depths less than 1000 meters, and similar results were obtained at a number of other stations in the area. From this evidence it may be concluded that the subspecies does not remain in the depth range 500-1000 meters, but merely passes through this region during its diurnal migration.

This study of the depth distribution of *Chauliodus* allows certain conclusions to be drawn. First, all the populations exhibit a vertical diurnal migration. The exception to this is, perhaps, *barbatus* which was not caught in the upper 500 meters in night tows. This may mean that this group does not migrate upward

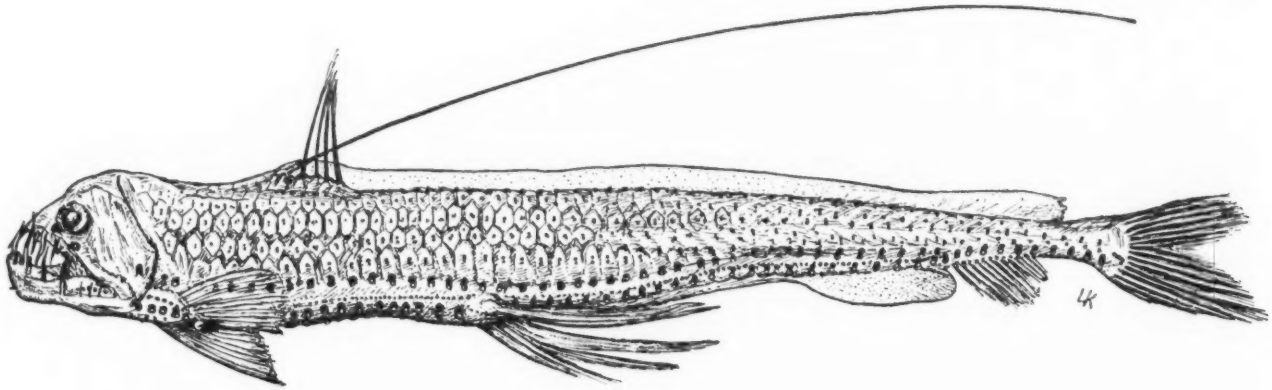


PLATE 1. *Chauliodus sloanei sloanei* Schneider. Drawing of a specimen taken by the *Pawnee* Expedition, 1927, in the vicinity of the Bahama Islands at a depth of about 2100 meters (7000 ft.) The standard length of this specimen is 65 mm. See text for description.

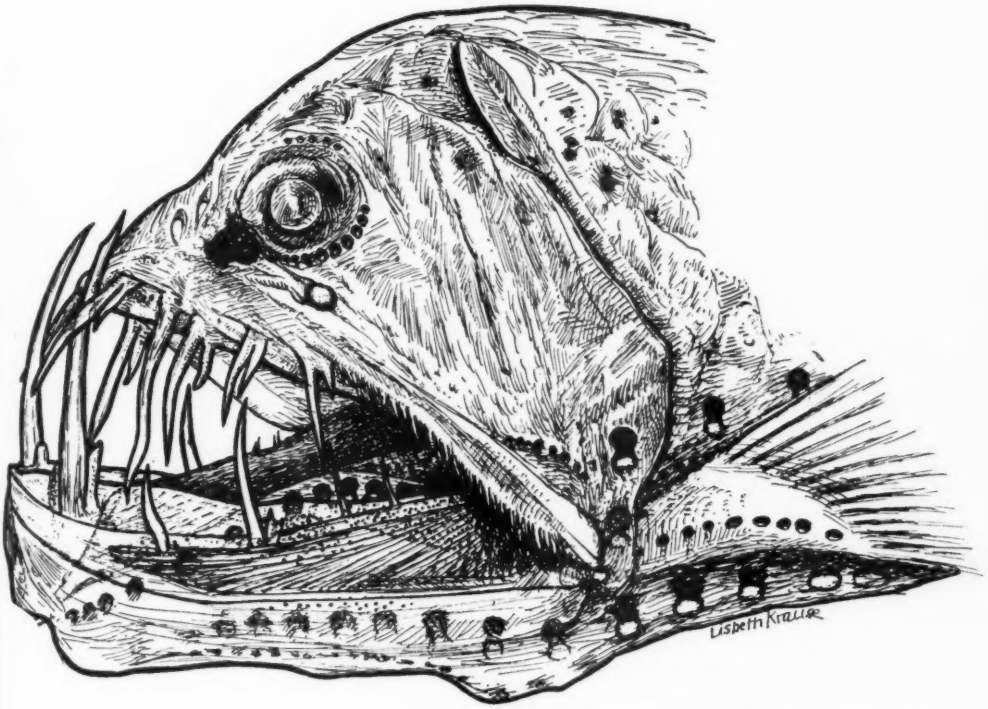


PLATE 2. Head of same specimen drawn at greater magnification. The teeth and photophores can be seen in greater detail. See text for description.

at night, but an equally possible interpretation is that this group does not migrate as close to the surface as do the other groups. Since the specimens taken were large (100-150 mm.) it is possible that *barbatus* is a larger species and, as was clear in the other species and subspecies, the large individuals do not come close to the surface.

A second point which comes out of this study but which is perhaps not as evident as the preceding one, is that where *s. sloanei* occurs together with another species or subspecies its vertical daytime range is considerably more restricted than in areas where it is alone (Fig. 4). Thus the depth distribution in the Indo-Malayan Archipelago is from 500 to 3000 meters during the day, while in the Indian Ocean in the areas where *s. sloanei* and *s. secundus* occur together, *s. sloanei* is distributed through only 600-1700 meters. The most pronounced limitation of daytime depth distribution of *s. sloanei* when occupying the area with another species or subspecies is found in the Atlantic Ocean. In the North Atlantic, where *danae* and *sloanei* are both found, *danae* has a daytime depth distribution of 500 to 2000 meters, whereas *s. sloanei* is limited to 1000-1800 meters. In the South Atlantic, although there is not much geographical overlap between *s. schmidti* and *s. sloanei*, the daytime distribution of *s. sloanei* is limited to 1000-1600 meters, while *s. schmidti* occurs from 400 to 3000 meters during the same hours.

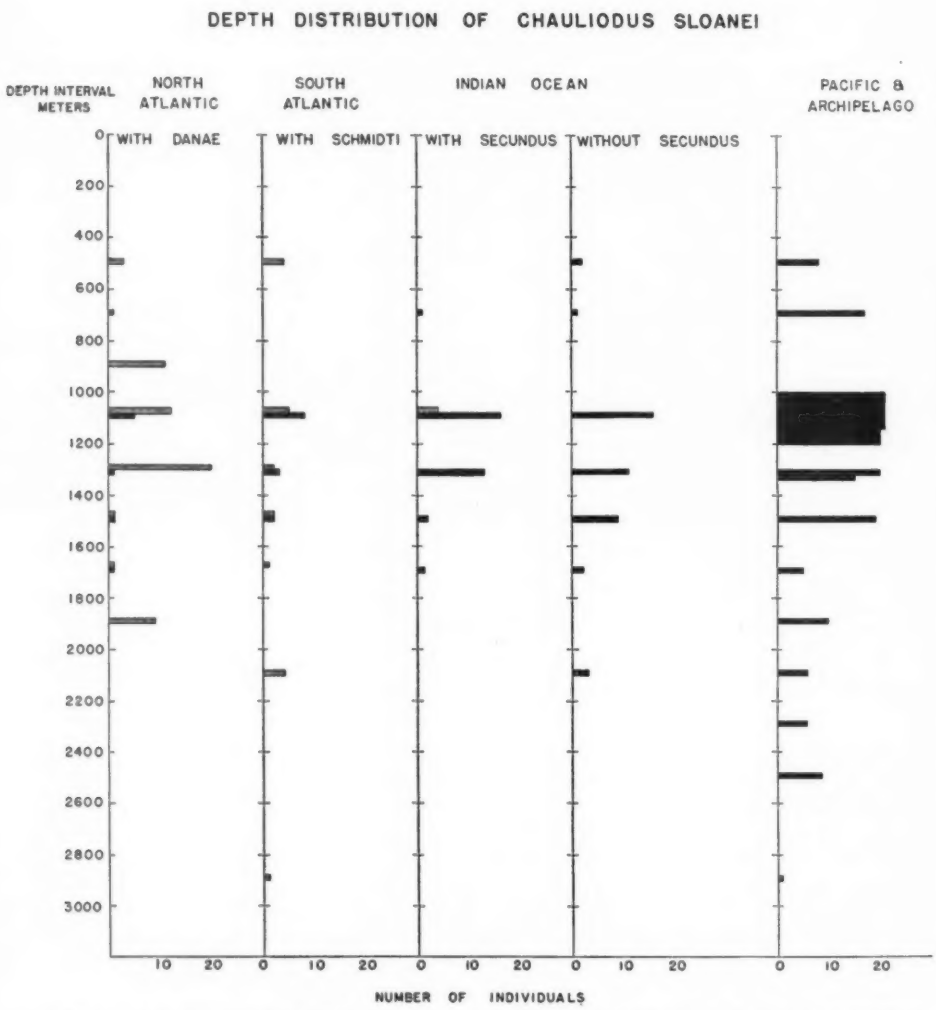


FIG. 4. Daytime depth distribution of *Ch. s. sloanei* in various ocean basins. The vertical range of this species appears to be restricted wherever another member of the genus occurs in the same basin. The black bars represent by their length the total number of *s. sloanei* caught within each depth interval, while the stippled bars represent the abundance of the accompanying species, the name of which is given at the head of each column.

Localization in Water Masses

A column of ocean water shows pronounced changes in temperature, salinity, and oxygen concentration from the surface to the bottom, and with our knowledge of the spatial distribution of the populations of *Chauliodus* it becomes possible to consider the environmental conditions under which they live and how these conditions may influence their distribution.

Attempts to correlate the distribution of the various populations of the Chauliodontidae with temperature alone yield inconclusive results (Fig. 5). This graph clearly indicates that all populations experience wide ranges of temperature and that these ranges overlap extensively. Neither the ranges of salinity nor the absolute values show any meaningful pattern.

Consideration of temperature and salinity together yields more interesting results. It is a general principle in oceanography that water acquires its temperature-salinity properties by changes at the surface. These changes are principally heating, cooling, evaporation, and precipitation, and they reflect the characteristics of the climate of the region. When the changes in the water are such that the density exceeds that of the underlying water, it sinks to depths corresponding to that density. Such bodies of water, with discrete characteristics of temperature and salinity are known as water types. As they move from their place of origin, they become mixed with other waters but the origin of their characteristic properties can still be recognized. The large bodies of water formed by this mixing are known as water masses. As an example of this process, the deep-water circulation of the Atlantic Ocean will be considered. Figure 6 shows a vertical section through the Atlantic, illustrating the formation of its major water masses. Starting at the Antarctic Continent, and proceeding north, we see that the very cold Antarctic Bottom Water sinks, while the Antarctic Deep Water comes to the surface to replace the water that descended because of its greater density. At about the 60° S parallel, the slightly warmer water from the north meets the colder water from the Antarctic region. This convergence gives rise to the Antarctic Intermediate Water, which, being slightly colder and therefore heavier than the water from the north, sinks. The Subantarctic Water *(SAA)* lies just north of the Intermediate Water, and contributes some water to its formation. The South and North Atlantic Central water masses are produced in a different manner. Here, instead of a convergence of two types of water, there is a general region of sinking, due to the density increase accompanying cooling and evaporation at the surface. These are rather large areas, which fluctuate in position and size with the seasons. They are larger and closer to the 35° parallels in the winter and smaller and closer to the poles in the summer. The Arctic Intermediate *(AI)* and the North Atlantic Deep and Bottom Water *(NADB)* are formed by the sinking of cold water. The Mediterranean Water *(M)*, flowing over the Gibraltar Sill and spreading out at mid-depths in the Atlantic, is warm but very saline. This water mixes with the Antarctic Intermediate and North Atlantic Central Water. The North and South Atlantic Central Waters will also mix slightly with

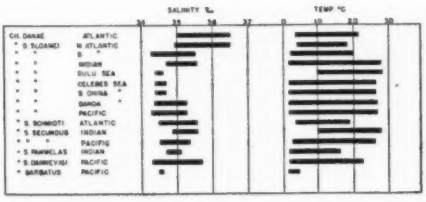


FIG. 5. Representation of the salinity and temperature characteristics of the waters in which the various species, subspecies and populations of *Chauliodus* live. Both the salinity and the temperature characteristics of the waters inhabited by any species or subspecies vary from basin to basin.

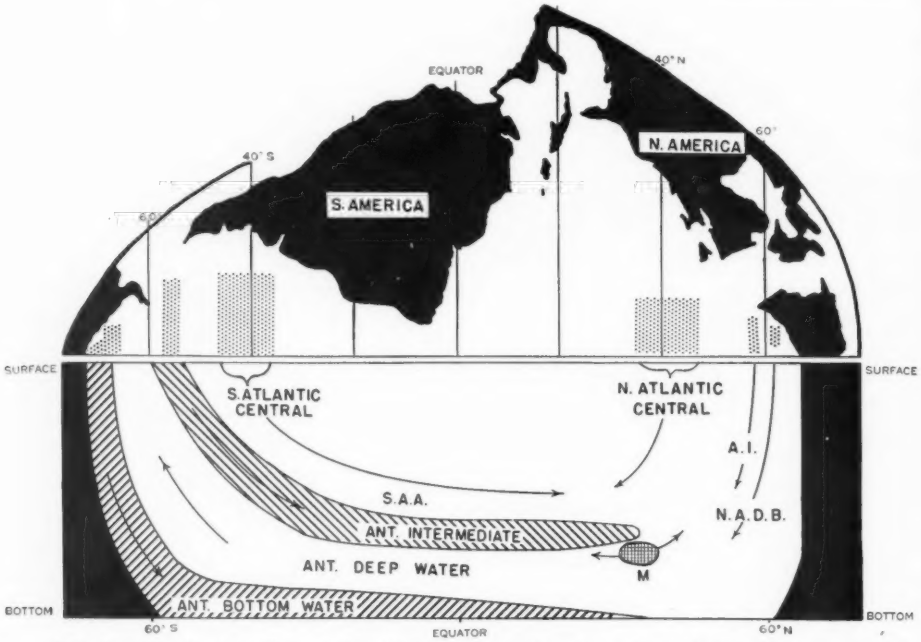


FIG. 6. A vertical section of the Atlantic Ocean showing schematically the formation of its major water masses. The stippled areas on the map indicate the location of regions of sinking. On the vertical section *AI* indicates Arctic Intermediate Water; *M*, Mediterranean Water; *NADB*, North Atlantic Deep and Bottom Water; *SAA*, Subantarctic Water. See text for explanation. Map based on Goode's Homolosine Equal-Area Projection, copyright by University of Chicago. Used by permission of Univ. of Chicago Press.

each other and with the Antarctic Intermediate. These water masses are, then, characterized by a more or less precise relationship between temperature and salinity. In practice, one of the best methods of identifying water masses is to plot their temperature-salinity relationships. When this is done, the water masses show up as envelopes with a characteristic shape, as illustrated by Figure 7. Having plotted the temperature-salinity characteristics of the major water masses of an ocean, it becomes possible to indicate the distribution of the various species and subspecies of any group with respect to the temperature and salinity of the water in which the specimens were taken and thereby to determine the water masses in which they live.

It is convenient to consider the distribution of the forms in each ocean basin

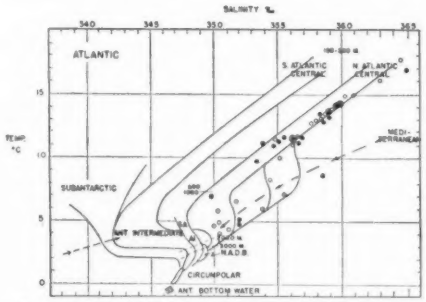


FIG. 7. The temperature-salinity characteristics of the waters of the North Atlantic in which specimens of *Ch. danae* (open circles) and *Ch. s. sloanei* (solid circles) have been found. The envelopes for the major water masses of the Atlantic Ocean are drawn. *AI*, Arctic Intermediate Water; *NADB*, North Atlantic Deep and Bottom Water; *SA*, Subarctic Water. Temperature-salinity diagram and envelopes from Sverdrup, and others (1942, Fig. 209B).

rather than follow the distribution of one species throughout its entire range. The Atlantic Ocean, which will be considered first, is inhabited by the species *danae*, and *sloanei*, the latter represented by the subspecies *sloanei* and *schmidti*. Figure 7 shows the distribution of *danae* and *s. sloanei* with respect to the water masses of the Atlantic. The diagram shows that *danae* is limited to the lower part, i.e., the colder portion of the North Atlantic Central Water. In the deeper parts of this water mass, the fish is found in that particular part which has been mixed with the Mediterranean Water. The species is not found in the North Atlantic Deep and Bottom Water (*NADB*) nor in the Antarctic Intermediate Water. It is also absent from the Subarctic (*SA*) and Arctic Intermediate *(AI)* water masses. The subspecies *s. sloanei* has a distribution similar to that of *danae*, but differs slightly from it in certain aspects. It is evident that *s. sloanei* is not nearly so narrowly distributed in the North Atlantic Central Water, and that it seldom occurs in that part of the water mass which comes under the influence of the Mediterranean outflow. This scarcity in the Mediterranean Water of the Atlantic is puzzling in view of the fact that there is a population of *s. sloanei* in the Mediterranean Sea, and it might be expected that the Atlantic population would be more or less continuous with it by means of the outflow over the Gibraltar Sill. Actually there are a number of slight morphological differences between the Mediterranean and Atlantic populations of *s. sloanei*. It is possible that *danae* is competitively keeping *s. sloanei* out of this region (as well as most of the western part of the Atlantic), although it has been unable to invade the highly saline and warmer water of the Mediterranean Basin.

In the South Atlantic the two forms to be considered are *s. sloanei* and *s. schmidti*. Figure 8 shows that *sloanei* is found in the N. Atlantic Deep and Bottom Water, in the Subantarctic Water, in the Antarctic Intermediate Water and S. Atlantic Central Water. The subspecies *schmidti* on the other hand, is not found in the N. Atlantic Deep and Bottom Water but does occur in the Subarctic Water and in that part of the Antarctic Intermediate Water which has been mixed with Mediterranean and N. Atlantic Central Water. Although *schmidti* is also found in the upper part of the S. Atlantic Central Water in about the same region as *sloanei*, only four specimens of *schmidti* were caught in the same net with *sloanei*. Three stations yielded both subspecies, but the fish were caught together in only three tows. In ten tows at these three stations, either one or the other subspecies was taken, and seventeen tows at the same stations produced no specimens of either. These three stations probably represent the area of overlap between these two contiguous subspecies, an area which is relatively devoid of each.

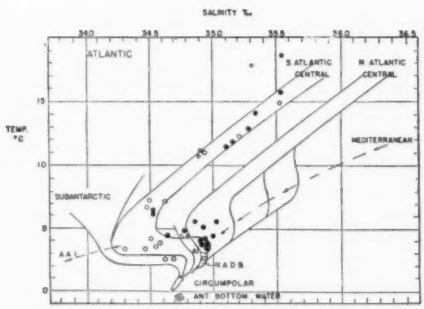


FIG. 8. Temperature-salinity properties of the water of the South Atlantic in which *Ch. s. sloanei* (open circles) and *s. schmidti* (solid circles) are known to live. (Note that in North Atlantic diagram, Figure 7, *s. sloanei* is indicated by solid circles.) Envelopes for major water masses as in Figure 7.

The fact that *danae* and *s. sloanei* do not have the same distribution in the North Atlantic Central water mass, and that *s. schmidti* has a rather limited horizontal distribution, although the South Atlantic Central Water and the other water masses in which it is found have a much wider distribution, suggest that some other environmental factor may be

instrumental in further influencing the distribution pattern of the Atlantic populations. Although water masses are recognized by their temperature-salinity relationships, it is well to remember that other properties, among them the nonconservative property of oxygen concentration will be found to vary in a predictable manner in the water mass, and it is conceivable that this environmental factor might be responsible for the distribution pattern.

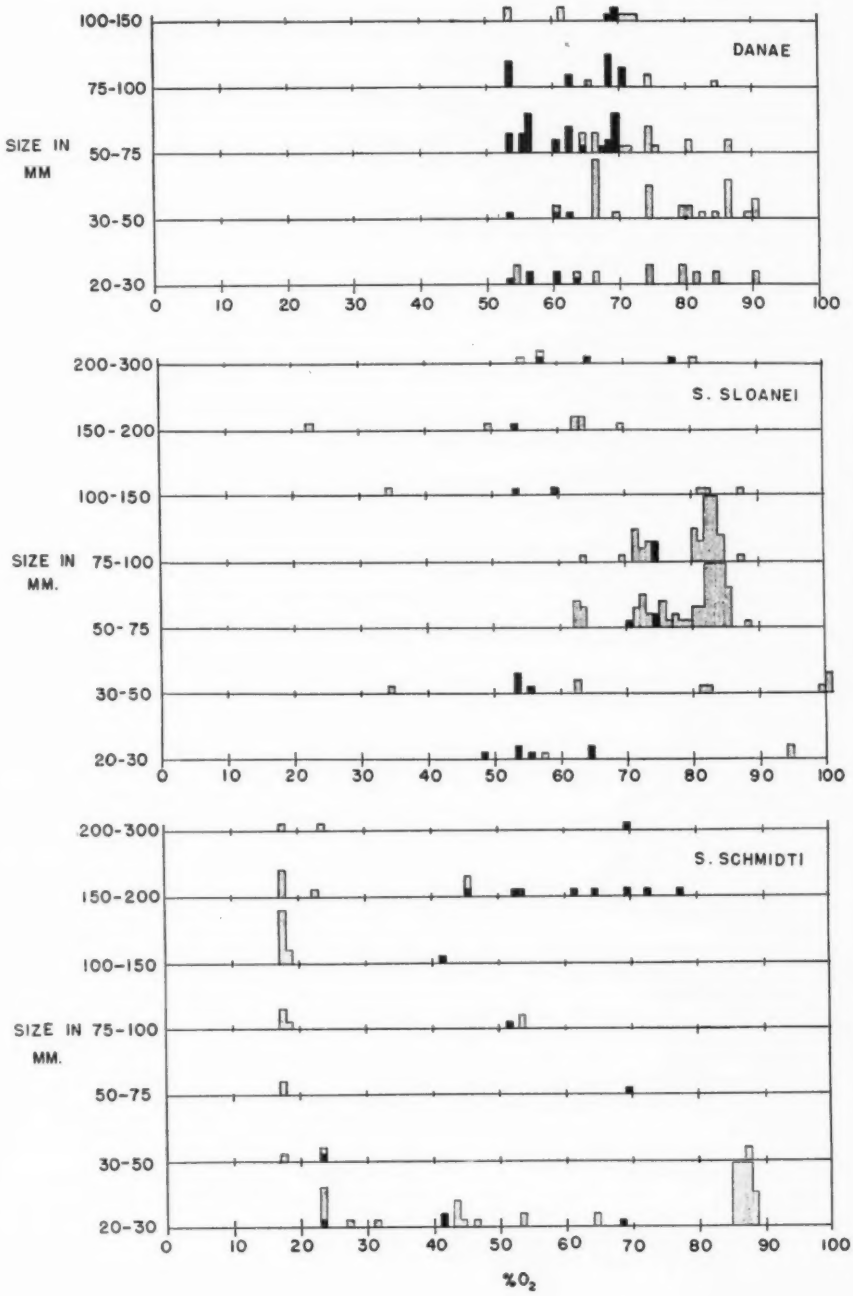
In order to determine the influence of high and low oxygen concentration upon the distribution of the various populations, the data were treated in the following manner. The different size categories of the individuals composing a population were plotted against oxygen saturation of the water. The degree of oxygen saturation indicates not only the absolute amount present but also the amount of biological activity there has been in that water. Night and day hauls were plotted separately. Figure 9 shows the three taxonomic groups of the Atlantic plotted with reference to the oxygen saturation of the water. This shows that *danae* is found in water with oxygen saturations of over 53 per cent. The diagram for *s. sloanei* indicates that this does occur in water less saturated with oxygen, but that the majority of the specimens were taken in water with a concentration as high as that in which *danae* occurs. Consideration of the graph of *s. schmidti* shows that although during the day these fish experience the same oxygen saturations as do *danae* and *s. sloanei*, at night the larger fish especially occur in water with very low oxygen. It will be recalled that these larger fish were mostly caught at about 500 meters by night.

This suggests that although *s. sloanei* is adapted to a wider range of oxygen concentrations than *danae* it cannot compete successfully with *danae* in the range between 53-75 per cent saturated. It also does not occur in regions where the oxygen concentration from 50-500 meters is very low, 17-28 per cent saturated. Only *s. schmidti* occurs there. Apparently neither *danae*, nor *s. sloanei* can compete with *s. schmidti* in this area, nor can *s. schmidti* invade other regions where the environmental factors would seem to favor the other two groups.

The water masses of the Indian Ocean have not been as thoroughly investigated as those of the Atlantic, and the distribution of the fish with respect to them must remain tentative. The Subantarctic, Antarctic Intermediate, and the Indian Central water masses are formed in ways similar to those described for the Atlantic, while the Indian Equatorial water which is not nearly as well defined as the other water masses, is formed by mixing of Red Sea Water, Antarctic Intermediate Water and other deep water. Figure 11 shows the distribution of *s. sloanei* in the Indian Ocean. The other two subspecies of *sloanei, secundus* and *pammelas* (which are not plotted), fall in the same water masses as does *sloanei*. Most of the points fall in the Indian Equatorial water mass although some are distributed in the Indian Central Water, mostly in that part of the Central Water with the same temperature-salinity characteristics as the Equatorial Water. Another population of *s. sloanei* seems to be restricted to water of narrow salinity range, this water being the result of the mixing of Indian Central Water with Red Sea Water on the one hand, and Antarctic Intermediate Water on the other.

The distribution of *s. sloanei* in the Indo-Malayan Archipelago provides an interesting study. The Archipelago consists

FIG. 9. The oxygen saturation of the waters of the Atlantic which *Ch. danae, s. sloanei,* and *s. schmidti* inhabit. The solid bars represent daytime catches; the stippled ones, nighttime. The height of a bar indicates the number of specimens of that particular size taken from water with the indicated percentage of oxygen saturation. A bar as high as it is wide represents a single specimen.


DANAE
S. SLOANEI
S. SCHMIDTI
SIZE IN MM
SIZE IN MM.
100-150
75-100
50-75
30-50
20-30
200-300
150-200
0 10 20 30 40 50 60 70 80 90 100
%O2

of numerous large and small basins, some of which are virtually landlocked (Fig. 10). The deep water and the surface water of the Archipelago comes from the Pacific, but the mid-depth circulation has not been studied in detail, so that the origins of the water masses of the mid-depths are uncertain. However, the distribution of the fish provides a clue to the origin of the mid-depth water in this area. The method used to determine the water masses was to plot the distribution of *s. sloanei* specimens taken in the area, first on a temperature-salinity diagram for the Indian Ocean, then on one for the Pacific Ocean. Figure 11 shows the distribution of the Celebes and Sulu Sea populations (open circles) when plotted on the Indian Ocean diagram. Clearly, most of the points fall outside any water mass envelope and so far from any definite water mass that it is questionable if the Indian Ocean contributes any mid-depth water to the Archipelago. It is true that the Celebes station plots indicate that it might be Indian Central Water mixed with Antarctic Intermediate Water which forms a part of the Celebes Sea. However, the depth relationships are sufficiently different to vitiate this possibility. The situation becomes clearer when the same stations are plotted on the Pacific Ocean diagram (Fig. 12). The Celebes Basin would appear on the basis of this diagram to receive most of its deep water from the Pacific Equatorial Water and some from the Western North Pacific Central Water by way of the Sulu Sea. The same water masses contribute to the basins of the South China Sea, Banda Sea, Molucca Passage and Makassar Straits. As might be expected, the morphological characters of the Archipelago populations are like those of the Pacific and not like those of the Indian Ocean. The Sulu Sea Water appears to have a very narrow salinity range, which may be interpreted as water which is formed locally, probably by mixing of Western North Pacific Central Water and Pacific Equatorial Water. The shallow sill depth of the Sulu Basin (400 meters) by preventing the inflow of cold deep water permits a temperature of 10° even at the greatest depth. In addition to the narrow salinity and temperature ranges of the Sulu Sea water, the oxygen concentration is similarly within a narrow range and the values are low, 17-38 per cent saturated.

FIG 10. The principal basins of the Indo-Malayan Archipelago. The 1000-meter depth line is indicated. The closed nature of the Sulu Sea is evident.

The Pacific subspecies of *sloanei*, *sloanei* and *secundus*, and the species *barbatus*, are all found in the Pacific Equatorial Water. In addition to inhabiting this water mass, *s. sloanei* is found in water which is a mixture of Western North Pacific Central and Pacific Equatorial water (Fig. 13). The distribution of *secundus* in the Pacific, based on the few points available, indicates that it is found in that part of the Pacific Equatorial Water which has the same temperature-salinity characteristics as those of the portion of the Indian Equatorial Water which the Indian Ocean populations inhabit. The distribution of *s. dannevigi* is clearly defined by the characteristic of the Western South Pacific Central Water Mass. It will be recalled that *s. secundus*

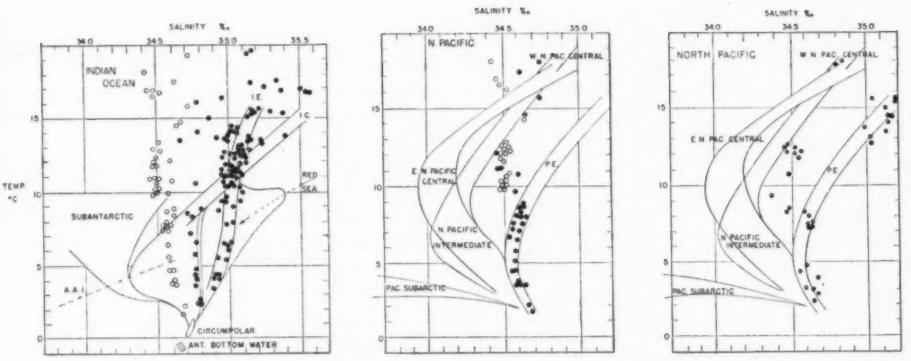


FIG. 11 (left). Solid circles indicate the temperature-salinity properties of water in which specimens of *Ch. s. sloanei* were caught in the Indian Ocean. Open circles indicate these for catches of the same subspecies in the Sulu and Celebes Seas of the Indo-Malayan Archipelago in an attempt to determine whether the waters in which these latter population live have the properties of any water mass of the Indian Ocean. The envelopes for the major water masses of that ocean are drawn. *IE,* Indian Equatorial Water; *IC,* Indian Central Water; *AAI,* Antarctic Intermediate Water. Temperature-salinity diagram and envelopes in Figures 11, 12 and 13 modified from Sverdrup, and others (1942, Fig. 209B).

FIG. 12 (center). A similar representation for specimens of *Ch. s. sloanei* of the Celebes Sea (solid circles) and the Sulu Sea (open circles). The envelopes for the major water masses of the North Pacific are superposed. *WN Pacific Central,* West North Pacific Central Water; *PE,* Pacific Equatorial Water.

FIG. 13 (right). A representation of the properties of the waters in which *Ch. s. sloanei* has been caught in the Pacific Ocean.

and *s. dannevigi* occur together in the vicinity of the Samoan Islands. However, the two subspecies occupy different water masses, *s. secundus* in the Equatorial Water and *s. dannevigi* in the Western South Pacific Central Water (Fig. 14). This nicely illustrates how two species whose areas of horizontal distribution overlap may be spatially isolated.

The oxygen concentration in the Pacific Ocean in the areas of *s. secundus, s. sloanei* and *s. dannevigi* is fairly high. Thus *dannevigi* is found in water with an oxygen saturation between 40 and 90 per cent. The gap in the vertical distribution between 500 and 1000 meters includes the oxygen minimum layer, and it is possible that this might explain the absence of the fish from these depths. This is not a likely explanation, however, because the oxygen minimum layer is poorly developed in this area of the Pacific, with absolute values between 3 and 4 ml./ liter. These represent a saturation of 50 per cent and similar values occur throughout a large part of the water column. The distribution of *s. secundus* with respect to oxygen falls within the same range as *dannevigi,* with perhaps a few more specimens at slightly lower oxygen values than *dannevigi.* The distribution of *s. sloanei* in the Pacific is slightly different from the two preceding groups. The smaller sizes, 20-70 mm., are found in a region of 60 to 80 per cent saturation, while the larger ones, 100-300 mm., are found in a layer with values of 29-40 per cent. This again suggests that *s. sloanei* is not able to compete with the other forms in the region of their narrow oxygen preferences, although better adapted for generally wide ranges of oxygen.

Discussion

One of the most careful studies of the environmental factors with which the distribution of a bathypelagic organism

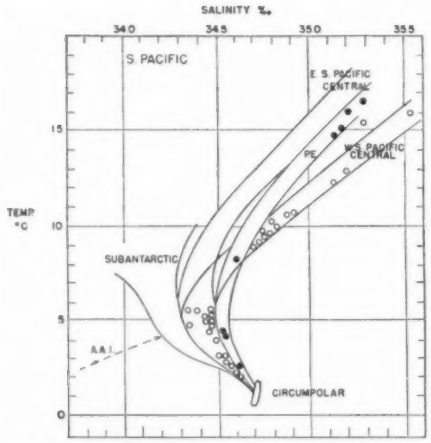


FIG. 14. Temperature-salinity characteristics of the waters of the Pacific in which *Ch. s. dannevigi* (open circles) and *s. secundus* (solid circles) live.

The major water masses of the South Pacific are indicated. *ES Pacific Central,* Eastern South Pacific Central Water; *PE,* Pacific Equatorial Water; *AAI,* Antarctic Intermediate Water. Temperature-salinity diagram and envelopes modified from Sverdrup, and others (1942, Fig. 209B).

might be correlated is that of *Vampyroteuthis infernalis* Chun by Pickford (1946). This dibranchiate cephalopod is stenothermic and stenohaline. The most constant environmental factor is, however, the density of the water in which it occurs, a correlation which was originally suggested by Dr. E. F. Thompson. All populations live in water between the narrow limits of 1.0274 and 1.0278 gm/cc, although such water may have different temperatures and salinities and lie at different depths in the Atlantic, Indian, and Pacific Oceans. Morphological studies (Pickford, 1949) reveal a tendency toward minor racial differences between the populations of the Atlantic and Indo-Pacific regions, but the range of overlap is so great as to preclude taxonomic recognition.

Evidence is presented in this study that the Chauliodontidae are restricted to water masses with narrow salinity-temperature relationships and oxygen concentrations. This allows some tentative conclusions to be drawn as to the role of the characteristics of the water masses in maintaining isolation between populations.

In the Atlantic the two species, *danae* and *s. sloanei,* are found together in the North Atlantic, in general occupying the same water mass, and with slight differences in the oxygen preferences. The general distribution of the two species suggests that *danae* is competitively excluding *s. sloanei* from certain areas. The South Atlantic *s. sloanei* and *s. schmidti* occupy different geographical areas, and where these overlap on the map, the fish are found in different water masses. The actual overlap which does occur, takes place in shallow depths at night and this mingling may be said to be limited to the smallest specimens. The subspecies *s. schmidti* seems to exclude both *danae* and *s. sloanei* from the regions where the water is of low oxygen content.

The Indian Ocean has three morphologically distinct groups, *s. pammelas, s. secundus* and *s. sloanei.* They overlap considerably in their horizontal and vertical distribution. Perhaps *s. pammelas* lives at greater depths, at least during the day, than the others, but so few catch records are available that this is doubtful. There is, however, no doubt that *s. secundus* wherever it occurs here is sympatric with *s. sloanei.* Both occupy the same water masses and have the same oxygen concentration ranges. The disjunct distribution of *s. secundus* shows most clearly its restriction to waters of certain temperature-salinity characteristics. The part of the Indian Equatorial Water Mass in which the subspecies is found has temperature-salinity characteristics which are like those of the Pacific Equatorial Water Mass in the area between the Samoan Islands and north of New Guinea. It is in this region that *s. secundus* again occurs. It will be recalled that *s. secundus* is not continuous across the Indian Ocean, but is found in two populations,

one around Madagascar and the other near the Nicobar Islands and in the waters between Sumatra and the Cocos Islands. A possible explanation is that *s. sloanei* has invaded the once continuous populations of *s. secundus* and is competetively keeping *s. secundus* out of these regions. There is no apparent reason why it should have forced *s. secundus* out of the areas in which it does not now occur, since the water masses and oxygen concentrations are the same for the entire area in which the two groups are found. Furthermore, the restriction of *s. sloanei* in depth when it occurs together with *s. secundus* indicates that *s. secundus* has an influence on the distribution of *s. sloanei.* Of course, there are other environmental factors associated with water masses which have not been considered here and it might be one of these that varies and allows the one or the other group to compete more successfully. Another possible explanation is that the Madagascar group is a terminal immigrant, having been transported there by the monsoon drift and not able to reproduce in this area. The morphological differences between the Madagascar and Sumatra populations are very small indeed when one considers the distance which separates them. The differences between the Indian Ocean and Pacific forms of *s. secundus,* however, indicate that the gene flow between these two populations is low.

The *sloanei* complex of the Indian Ocean, Indo-Malayan Archipelago, and the Pacific is no less difficult to analyse. The Indian Ocean population of *s. sloanei* is morphologically distinct from those of the Indo-Malayan Archipelago, and within the Archipelago the Sulu Sea population is different from those of the rest of the basins. In addition, the Pacific form, although resembling those of the Celebes Sea, Banda Sea and other basins of the Archipelago, still has some morphological characteristics that are different. These morphologically distinct populations of *Chauliodus* in this region suggest that many of the habitats were more isolated in the past than they are at present. It is doubtful that the Pleistocene glaciation had much effect on the temperature of the water masses. The salinity gradients would probably have been slightly steeper, but the general circulation of the oceans (with the possible exception of the Pacific) would probably have been the same, even if compressed toward the equator. The Pleistocene glaciation did, however, lower the mean sea level, and in the Archipelago a drop in the sea level of 100 to 200 meters would more nearly isolate many basins thereby increasing the isolation of populations of a fish such as *Chauliodus* which requires a considerable depth of water.

The Indian Ocean *s. sloanei* is found in the Equatorial Water but the Pacific form is not found in that part of the Equatorial Water that has the same characteristics as the Indian Equatorial Water as was the case with *s. secundus.* Rather, it is that part of the Pacific Equatorial Water which is found in the Archipelago, an indication, perhaps that *s. sloanei* of the Archipelago has only recently invaded the Pacific. As would be expected under such circumstances, the morphological characters are the same for the Pacific and Archipelago populations, indicating a gene flow between the two, whereas the Indian Ocean group differs from the Indo-Malayan–Pacific groups.

The above discussion does not apply to the Sulu Sea populations. This basin can be considered as being locally formed water, with temperature-salinity characteristics which differ from those of the surrounding bodies of water. Having different temperatures and salinities, and also different oxygen concentrations, and being virtually landlocked, the Sulu Sea provides an isolated and ecologically distinct habitat. The degree of isolation is attested by the morphological differences between this population and those of neighboring waters.

The subspecies *dannevigi* is found only in Western South Pacific Water, and al-

though there is an apparent overlap with *secundus* at the Samoan Islands, the two groups inhabit different water masses.

In a discussion of the Pacific groups, the question arises as to the extent of the populations in this ocean. The Pacific is the least investigated area, and it is possible that the large gaps represent areas not sampled. This may be true to some extent, but the distribution of the populations within certain water masses indicates that the animals will be found in the water mass as long as it is at a depth where the deep scattering layer will fall within its range. The water mass cannot be too deep or too shallow or the fish will not be able to live in it even though the temperature-salinity-oxygen requirements are fulfilled. With this in mind, we can speculate that *barbatus* will probably be found in an area from the Gulf of Panama to the Fanning, Christmas, Palmyra Islands group, and not outside the Pacific Equatorial Current. The *s. dannevigi* population may possibly extend its area south of its present known distribution, but not as far south as the Antarctic Convergence. The *s. macouni* population is probably well delineated by its known distribution. If further investigations of the Pacific should produce fish from areas for which we have no records at the present time, it is reasonably certain that they will differ morphologically from the known populations.

The only populations which should be wider in distribution, on theoretical considerations, are the two species of the North Atlantic, *s. sloanei* and *danae;* the range should extend north almost to Greenland and along the east coast of South America.

This study has been an attempt to discover some of the ecological factors which influence the distribution and speciation of mid-depth fish. At least a beginning has been made in delineating some of the factors important to one organism in this part of the biosphere. It is hoped that technological advances such as high-speed trawls, more accurate sonic depth recorders and bathygraphs will produce more accurate data and larger samples of fish so that some of the vague elements can be resolved into a clearer picture.

Summary

1. The factors determining the distribution of closely related species of terrestrial organisms have received considerable attention; but comparable factors in the apparently continuous bathypelagic region of the oceans are little understood. In this paper the distribution of the three species and six subspecies of the clupeoid genus *Chauliodus* has been examined in an attempt to elucidate these factors.
2. The catch records of the *Dana* expeditions have been re-examined and it appears that most specimens live between 500 and 2500 meters by day and at depths of less than 500 meters at night. This would indicate an extensive vertical migration.
3. Further evidence for this migration is drawn from the fact that *Chauliodus* is shown to belong to that assemblage of organisms known as the "Deep Scattering Layer," a large part of which is known to undergo such diurnal migration.
4. It is shown that each population is restricted to certain water masses identified by their temperature-salinity characteristics.
5. From a consideration of depth distribution and water-mass localization it appears that most populations are allopatric. Although the three Atlantic populations overlap in part, the major concentrations of each occupy different regions.
6. However, *Ch. sloanei secundus* of the Indian and Pacific Oceans is always sympatric with *Ch. s. sloanei,* although in many parts of these oceans the latter occurs alone.

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Preparing and Photographing Slides of Insect Genitalia

R. R. DREISBACH

IN STUDYING the family Psammocharidae (Hymenoptera: Vespoidea), examination of the male genitalia was found to be necessary for distinguishing species. Most of the small specimens in this family seem to have no definite external distinguishing characteristics; at least none were discernible until the genitalia were examined and species with similar genitalia set into groups. Since the making of drawings consumes an enormous amount of time, and since drawings seldom, if ever, represent the genitalia as they actually are, it was decided to take photomicrographs. The procedure used in extracting the genitalia, making the slides, and taking the photomicrographs is herewith described. The method may be of interest to other systematists.

Pinned specimens are relaxed on a pinning block placed over moist sand enclosed in a bell jar, the whole set in a warm location. A simple type of relaxing jar is made by taking a half-gallon or gallon bottle and cutting it in two. The top part is used with the bottle opening corked. This jar is then placed over moist sand on a large plate. If the temperature is high enough to cause moisture to settle on the sides of the glass, sufficient relaxing occurs overnight. However, in a colder place it probably would take at least 24 hours to relax a specimen sufficiently.

After the specimen is relaxed, the pinned wasp is held in the left hand under a binocular dissecting microscope magnifying about 25 times. The genitalia and the subgenital plate are extracted by means of a No. 00 or 1½ insect pin which has the point slightly bent. In most cases the genitalia can be removed without damage to the specimen, although the very smallest specimens, only about one-fifth of an inch in length, are rather difficult to handle.

Six small test tubes are set up in holes bored in a block of wood, and genitalia are extracted from six specimens at a time; a number identifies each test tube and specimen. A few milliliters of 10 per cent caustic soda (sodium hydroxide) are then put in the test tube, and the solution is brought to a boil, great care being taken to avoid the loss of the contents through "bumping." This bumping is eliminated by continuous shaking of the test tube while it is heated over a small alcohol lamp. Instead of boiling the caustic, it may be allowed to stand about 10 to 12 hours at room temperature, but no longer. If the genitalia are not immediately removed from the boiling caustic or after standing 10 to 12 hours at room temperature, so much material is dissolved that the genitalia are hardly visible when mounted.

The genitalia and subgenital plate are removed to a small petri dish containing boiled water. It is necessary to use boiled water as this is free of dissolved air. Otherwise air bubbles may form and stick to the genitalia, causing them to float; or air bubbles may become trapped in different parts of the genitalia, making it almost impossible to remove them without tearing the genitalia apart. The extraneous parts are teased off by means of two of the bent pins mentioned above, then transferred to 90 per cent, 95 per cent, and absolute alcohol and to xylol, and mounted on a slide in Clarite or similar mounting medium. Some Clarite is

placed on the slide, and the genitalia are transferred from the xylol into it and oriented by means of the bent pins while being observed through the binocular microscope. The genitalia and the subgenital plates are always placed with the ventral side up. The amount of Clarite or other mounting medium needed depends on the size of the genitalia. The experience obtained from making a few slides will indicate the right amount. After the specimen is oriented on the slide, the cover glass is placed so that one edge rests on the slide a slight distance from the Clarite solution and then is slowly lowered into contact with the Clarite. If the slide is covered in this manner, no bubbles form under the cover glass. In order to make slides in which the preparations are comparable, it is necessary to take great care in applying the same pressure to the cover glass in each case.

The genitalia of these wasps make beautiful slides, for, although in their natural position in the wasps they are somewhat doubled up, they can be pressed out so as to lay almost in a plane without distortion. With some experience slide after slide of the same species can be made so as to have exactly the same appearance, and with very little variation within a species. As far as the present writer is concerned, it is absolutely impossible to study genitalia of this type and size without making slides, at least for the first specimens studied. It is sometimes possible thereafter to make correct determinations by simply extracting the genitalia and subgenital plate and comparing them with the original slide. However, until considerable experience is gained it may be necessary in closely related species of critical genera to make many slides since some characters are hardly visible except in mounted material. It is almost a necessity to make slides, at least originally, in the study of the Psammocharidae. Moreover, for future work the slides are immediately available.

Too many drawings have been made from the genitalia lying loose in a receptacle and viewed in a microscope, or from genitalia which have been torn apart. In the latter case the drawings are all but worthless for future use since the work entailed makes it impractical. (Further, these small parts are often lost in handling. Also, if the genitalia and subgenital plate are not mounted, the specimen moves around in the liquid and it is almost impossible to keep them in one position. Otherwise, if the genitalia are placed in a small vial and pinned on the same pin as the insect, it is almost always a chore to remove them from the vial for study. During this handling they may be lost or air bubbles, which are almost impossible to remove, may become attached to them.) Owing to the difficulty sometimes encountered in extracting them, it is almost impossible to have the unmounted genitalia appear the same way on each examination. Sometimes they are extracted almost in the position in which they lie in the abdomen; in other cases they are partially torn apart and flattened out. Under these conditions they will not look the same on repeated examination and errors of judgment may be introduced. Sometimes it may be that the variations represent variations within the species, while in other cases the two preparations may be considered to be genitalia of different species.

Many of the published drawings of the genitalia of the Psammocharidae do not show the proportions correctly. There may be convexities or concavities in the wrong places. It seems to be all but impossible to reproduce what is shown on the slide even if one is a competent artist; but a photograph reproduces exactly what is there. Working at top speed it is possible in an eight-hour day to extract the genitalia and subgenital plates and make slides of 36 specimens. It would take an enormous amount of time to draw these, whereas after the appropriate equipment is set up, 40 to 50 photomicrographs can be taken per hour very easily. The monocular microscope and attached film holder which the author has used are

shown in Figure 1. If the stage of the microscope has a turntable, the slide can be turned so that the specimen is right side up and perpendicular to the viewer. It is convenient to have a side arm on which can first be mounted a ground glass for moving the object into the field, and then a clear glass, on which with a magnifier the genitalia are brought into focus. Then all that is necessary is to snap the shutter.

The light is provided by a 100-watt bulb in a gooseneck lamp placed close to the reflector near the stage. A lens below the stage of the microscope softens the light and eliminates glare. This permits much more accurate focusing. Without this lens 1/10 second was used. With the lens enough light is cut off so that it is necessary to use 1/5 second to get the right exposure. The 1/5 second is satisfactory even with higher magnifications. The film used was Panatomic "X," the finest grained film available. Sample photomicrographs are reproduced as Figures 2 and 3.

The inclusion of the subgenital plate with the genitalia makes it a simple matter to separate most species of psammocharids. Where the genitalia are so similar that it may be doubtful whether they represent different species, the subgenital plates may be so radically different that there is no question but that the specimens are distinctly different species. As many as 70 specimens of one species which ranges from Maine to Vancouver in the Canadian zone have absolutely identical genitalia and subgenital plates with no noticeable variation whatsoever.

There have been a great many students of the Tachinidae, Muscidae, Sciomyzidae, Elateridae, and other families, who have found that it is absolutely necessary to study the male genitalia in order to distinguish species in certain genera. Further, in some genera of these families the species have not been found to have any distinguishing characteristics in the imago. Apparently the same is true of some genera of the Psammocharidae, and to say that specimens with very perceptible and distinct differences in the genitalia and subgenital plate are the same species just because no distinctly different characters can be found in the imago is not necessarily a valid argument. In these difficult genera the writer has found no way to relate the males and females in many species. In fact, the females in some genera are not definitely separable. Up to the present, no characters can be found in the female genitalia useful for separating the species in the critical genera.

R. R. DREISBACH of Midland, Michigan, an industrial chemist by profession, is a well-known authority on the spider wasps about which he writes in this informative contribution.

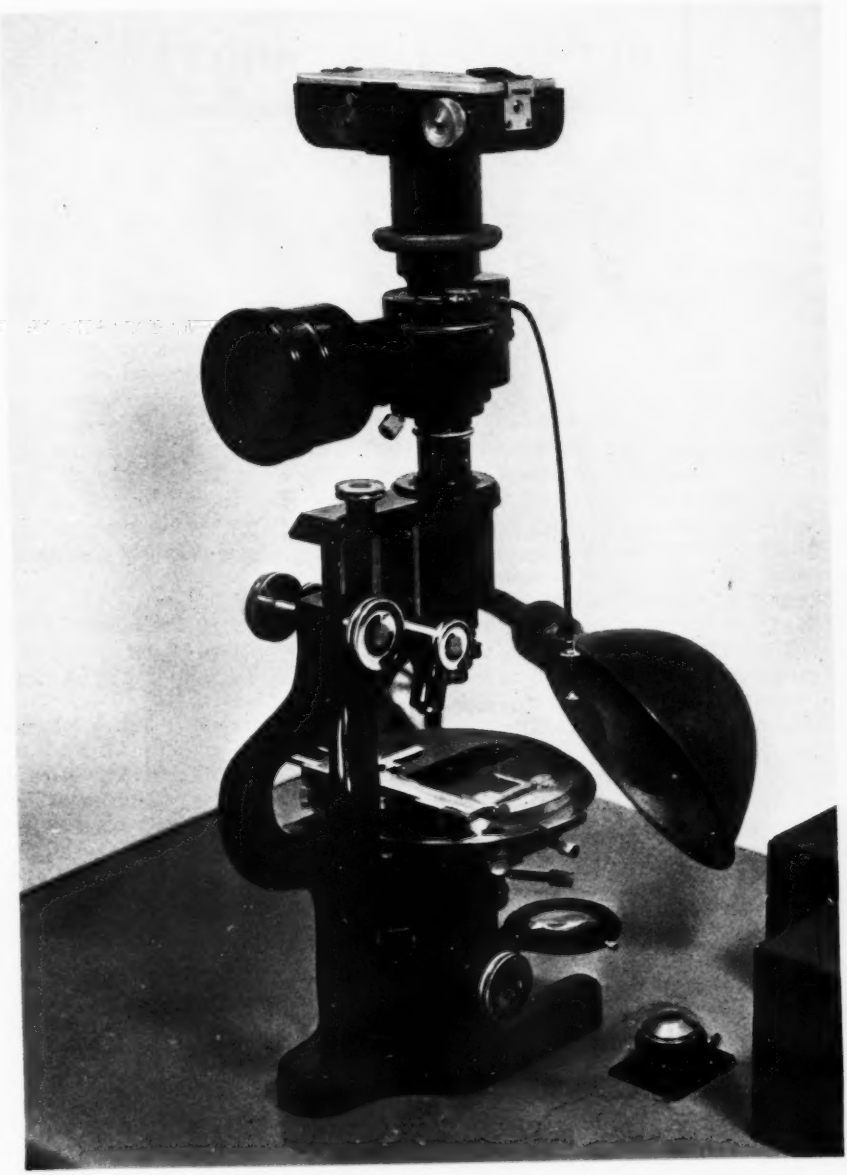

FIG. 1. The microscope and attached film holder, the use of which is described in the text. Figures 2 and 3 are photomicrographs taken with this apparatus.

FIG. 2. Photomicrograph of the genitalia of *Auplopus architectus* (Say), (Hymenoptera: Psammocharidae). Magnification 80 X. Procedure described in text.

FIG. 3. Photomicrograph of subgenital plate of *Auplopus architectus* (Say). Magnification 100 X.

People and Projects

Henry B. Bigelow of Harvard University, a member of the Society of Systematic Zoology, was awarded the Daniel Giraud Elliot Medal (for 1948) at the annual general meeting of the National Academy of Sciences. Dr. Bigelow received this award for his senior authorship of "Fishes of the Western North Atlantic, Part I," published in that year by the Sears Foundation for Marine Research. Lancelets, cyclostomes and sharks are the subjects of the three parts of this volume. This gold medal is presented annually to the author of that work in zoology or paleontology which the Academy considers the most meritorious of the year.

Carl L. Hubbs was elected to membership in the National Academy of Sciences at their April meeting.

One of the three American members of the International Commission on Zoological Nomenclature, J. Chester Bradley, has retired from the Department of Entomology at Cornell University. He has been active in the affairs of the Entomological Society of America from its founding, serving as its first secretary-treasurer and last year as its president.

The ocean's deepest sounding (5940 fathoms) was recently taken in a trench between Guam and Yap by HMS *Challenger*. This twentieth century *Challenger* is carrying on the work begun by her predecessor in the last century. The ship is equipped with modern echo-sounding gear, bottom samplers, and the apparatus for seismic refraction and reflection developed by Hill and Swallow for deep-ocean use. The *Challenger* left the Pacific area in April, to return to England via the Indian Ocean and the Mediterranean.

Dr. Theodor Mortensen, outstanding Danish zoologist and authority on echinoderms, died on April 3, at the age of eighty-four. Fifty-five of these years had been devoted to a study of echinoderms, particularly of sea-urchins. In 1897 when Mortenson began his work on the echinoids no fundamental advances in their classification seemed possible, based as it was upon the coronal and apical plates. The wealth of material provided both by expeditions in which he took part and by those with which he collaborated (Danish *Ingolf*, German Plankton, German and Swedish South Polar and American *Albatross*) enabled him to revolutionize the taxonomy of this group. He broadened the basis of classification by considering the structure of many of the hard and soft parts of the adult, and the development of the pluteus larva in addition to the characters previously in use. From 1927 until shortly before his death Mortensen was largely occupied with a "Monograph of Echinoidea."

In the Clyde Sea area off the west coast of Scotland a surprising number of polychaete annelids, amounting to four or five per cent of the species known from these waters, show morphological differences from what are considered the typical forms. Three of these populations are considered specifically distinct, four are considered varieties and three others show differences too slight to be awarded nomenclatural distinction. R. B. Clark (*Nature, 169*,975) makes the interesting suggestion that the loss of pelagic larvae by all but one of these variant forms (if the breeding habits reported in the literature apply to the Clyde Sea forms) would permit the establishment of small interbreeding populations. In such isolated

populations morphological divergence might be expected. If this suggestion is correct it may offer an explanation of the great divergence within the families of polychaetes, and should be considered in explanation of the evolution of some of the major groups of invertebrates.

The third volume of Résultats scientifiques de l'Exploration hydrobiologique du lac Tanganika (1946-1947) is now available. The first part of this volume comprises Lamellibranches, by E. Leloup, 300 francs (Belgian). The second contains Trematoda, Cestoda and Acanthocephala by S. Prudhoe; Coleoptera Carabidae, by P. Basilewski; Bryozoaires, by A. W. Lacourt; Méduses, by E. Leloup; and Cyclopides (Crustacés copépodes) by K. Lindberg. Price 200 francs. These can be ordered from the Secretary of the Commission Administrative du Patrimoine de l'Institut Royal des Sciences Naturelles de Belgique, Rue Vautier 31, Brussels 4, Belgium.

Copies of a symposium on explosive evolution entitled "Distribution of Evolutionary Explosions in Geological Time" are available from the Paleontological Society. This 97 page reprint from the *Journal of Paleontology, 26,* 1952, contains seven principal addresses and a general discussion. At $2.00 per copy ($1.75 each for five or more copies) the symposium may be ordered from the secretary, Kenneth E. Caster, Department of Geology and Geography, University of Cincinnati, Cincinnati 21, Ohio. Order should be accompanied by remittance.

Steps have been taken to foster a series of publications on Pacific Northwest insects and their near relatives by setting up an inter-institutional committee consisting of representatives from the following schools: University of Washington, University of British Columbia, Washington State College, University of Idaho and Oregon State College. Several taxonomic workers are now preparing manuscripts to contribute to the series. It is planned to make the publications alike in size, format and general content. This will make it possible for the various publications to be filed together in order as they appear. The above institutions will collaborate in the publication of the series. Several institutional and private collections are available for the use of specialists working on the studies. Contributors will be expected to make full use of such available material as well as published records in the preparation of publications. It is anticipated that the project will carry on over many years, partly because of the breadth of the project and partly because of lack of taxonomists in some groups. Further information is available from Herman A. Scullen, Oregon State College, Corvallis.

Ernst Mayr has accepted appointment as visiting professor at the University of Washington, Seattle, to teach a course in systematics and evolution.

We note with regret the deaths of the following members:

Harold Kirby of Berkeley, California, specialist on protozoa, February 21, 1952.

James Lee Peters of Cambridge, Massachusetts, specialist on birds of the world and president of the International Commission on Zoological Nomenclature, April 19, 1952.

The Society recently received the following letter from a member: "Enclosed please find a one-dollar bill ($1.00), my dues for 1952; and, also accept my resignation from the above Society, as its principles do not appeal to me. We need more old-fashioned constructive efforts, not the communistic 'tearing down of all old established rules and methods'. . . . Very truly yours."

SSZ News

THE CORVALLIS MEETING AND THE PROPOSED PACIFIC SECTION

During the past year plans have been under way to form an active section of the Society among its far western members. The plan was first discussed at the Los Angeles meeting of the Pacific Section of the AAAS in June 1951 under the leadership of Carl L. Hubbs, at which time John S. Garth, of the Allan Hancock Foundation was asked to serve as secretary pending formal organization. An organizing committee with Dr. Garth as chairman in Dr. Hubbs' absence met again in December and planned a full participation in the June 1952 meetings, of the Pacific Section of the AAAS at Corvallis, Oregon. This meeting included a symposium, a luncheon, a business meeting, a session for contributed papers, and the exhibit of zoological books from the library of the Society.

The following reports of this meeting have been received:

Symposium: Problems of Nomenclatorial Practice now under Consideration by the International Commission. W. I. Follett, presiding.

1. USINGER, ROBERT L., University of California at Berkeley: "The Question of Whether, and Subject to What Conditions, the Concept of a 'Neotype' Should be Officially Recognized in the *Règles*."

Dr. Usinger reviewed the historical definitions of the neotype, beginning with that of Kossman (1896): "a topotype figured or described to replace a specimen which has been lost or destroyed," and later modified to read: "a specimen, not necessarily a topotype, figured or described to replace a specimen which has been lost or destroyed." With this he contrasted the definition proposed by Hemming (*Bull. Zool. Nomen., 7,* 140 1952): "a final and irrevocable standard for the identification of a species in cases where there is no surviving type material or where the known surviving type material is insufficient for this purpose."

In the discussion that followed, G. F. Ferris agreed with Dr. Usinger that the first definition, as later modified, is the one most generally understood by systematists when the term "neotype" is mentioned, and that the definition proposed by Hemming was unwise, in that it opens new views and permits individual interpretations of what constitutes insufficient surviving type material.

M. W. de Laubenfels raised the question of neotypes of genera, which he believes are needed in his work on sponges.

J. S. Garth spoke in opposition to any requirement that neotypes be deposited in the institutions which had custody of the original types prior to their destruction. He pointed out that west coast crustacean types of A. Milne Edwards, Stimpson, Lockington, and Cano were destroyed by a series of catastrophies, and that if Pacific coast workers were to be held to such a provision, they would be working almost exclusively for the benefit of institutions other than their own and in most cases remote from the present centers of activity on Pacific coast Crustacea. Mr. Ferris agreed that such a stipulation was unwise and unnecessary.

With respect to the feared exploitation of the neotype (Hemming, 1952, 135) and the consequent expressed desire that the sole right to designate neotypes be vested in the International Commission, Martin R. Brittan inquired whether the Commission has a staff of specialists competent to adjudicate these matters. Dr. Usinger replied that, while the principle

of consulting specialists is well established, the number of petitions is already great and the addition of the responsibility of designating neotypes would, in his opinion, seriously overload the Commission and its consultative machinery.

Concerning the proposed stipulations (Hemming, 1952, 137, 138) that the neotype should conform to the original description and be from the same locality as the type, Dr. Usinger suggested that "the neotype be not inconsistent with the original description with respect to type locality, etc." He also was of the opinion that the law of priority would suffice in cases involving possible duplicate neotypes. (Hemming, 1952, 135).

Regarding the statement (Hemming, 1952, 140) that the neotype, once established, should take precedence over all surviving type material, Dr. Usinger affirmed that there should be no surviving type material; otherwise, there would be no necessity for establishing the neotype. The case of loss of the holotype with subsequent rediscovery was raised, Mr. Ferris being firmly of the opinion that a neotype proposed under these circumstances should lose its validity upon such rediscovery of the lost holotype.

Instead of the one year of public notice that would be required by Hemming (1952, 141) before a neotype could be designated as such, Dr. Usinger proposed that a one-year period of protest be allowed subsequent to publication of a neotype designation. This proposal met with general approval among those present.

The proposal for the restriction of the type locality (Hemming, 1952, 139) was discussed by Dr. Brittan, who raised the question of extinction of the species in the locality from which the lost type came, and its survival in some other locality. To this Dr. Usinger opined that the present trend was away from restricting type locality in the designation of neotypes.

The distinction made between official and unofficial neotypes and the implication that the latter would have no standing under the proposed new system, (Hemming, 1952, 146) drew forth some of the sharpest comment of the day. It was generally felt that to declare null and void all neotypes proposed before the date of inauguration of the new system would undo much careful work by many competent specialists who have preceded their colleagues (and the International Commission) in recognizing the need for neotypes, and in establishing them.

Although no vote was taken, it appeared to be the consensus of those present (1) that neotypes should be recognized; (2) that if the original type were rediscovered, it should take precedence over the neotype; (3) that the concept of neotypes should not be extended to supplement inadequate types, but only to replace lost types; (4) that neotypes should not be subjected to the exclusive jurisdiction of the International Commission; and (5) that the suggestions embodied in Dr. Usinger's critique of Mr. Hemming's views are generally acceptable.

2. DOUGHERTY, ELLSWORTH C., University of California at Berkeley. "The Application of a Trivial Name Which, When First Published, Is Applied to a Particular Species or Specimen, but Which is Stated also to be a Substitute Name for Some Previously Published Name."

3. DOUGHERTY, E. C.: "The Species to be Accepted as the Type Species of a Nominal Genus, the Name of Which Was First Published in a Generic Synonomy."

In the absence of Dr. Dougherty (who is about to re-enter the U. S. Air Force), these two papers were read by Joel W. Hedgpeth, of the Scripps Institution of Oceanography, La Jolla, California. Since the papers were highly technical and Dr. Hedgpeth did not feel qualified to answer questions concerning them, discussion was waived by common consent.

4. FOLLETT, W. I., California Academy of Sciences, San Francisco: "The Reform of Article 19 of the *Règles* Relating to the Conditions in Which Emendations of Scientific Names Should Be Made or Accepted."

Under the chairmanship of G. F. Ferris,

discussion of this paper first centered around Mr. Follett's suggestion that the original orthography be preserved unless emended by the International Commission; and that, as between variant spellings in an original description, the original orthography should be determined by position-precedence, except (possibly) where an express corrigendum was published as a portion of the same volume.

Mr. Hedgpeth cited a pertinent case among the pectens, and Dr. de Laubenfels mentioned two cases among the sponges. In one of the latter, the variant spellings *"Dusidea"* and *"Dysidea"* occurred in the same paper; in the other, the spelling *"Hircinia"* was substituted for *"Ircinia"* in a subsequent paper after the lapse of one year.

Mr. Follett believed that a sacrifice of the automatic emendation of "evident" or "demonstrable" errors did not seem too high a price to pay for objectivity, and that he could see no manner in which a rule could be worded to permit automatic emendation in extreme cases without permitting a subjective interpretation of the "evidence" in other cases.

Dr. Usinger stated his preference for a rule that would permit the emendation of an "evident typographical error" without reference to the International Commission, or in the alternative, for the proposal of Blackwelder, Knight, and Sabrosky (mentioned in Mr. Follett's paper) that would permit emendation by subsequent authors if "it can be demonstrated in the original publication itself that there has occurred an inadvertent error, such as a lapsus or a copyist's or printer's error."

Mr. Follett stated that there is a considerable gradation of "obvious" errors, accompanied by varying amounts of "evidence" (direct or circumstantial) in the original description. Toward one end of the series might be placed the trivial name *"hopiknsii."* It was accompanied by "evidence" which "demonstrates" that the species was "named for Timothy Hopkins." Toward the other end of the series might be placed the trivial name *"lahonton."* It was accompanied by "evidence" which, it has been contended, "demonstrates" that the author intentionally chose the spelling "Lahonton" for Lake Lahontan. But it has also been contended that the accompanying evidence "demonstrates" that the spelling *"lahonton"* was a lapsus or a copyist's or printer's error. The question which "demonstration" shall prevail appears highly subjective.

Dr. de Laubenfels mentioned two genera named for the same man, in which the name was spelled differently, and he pointed out the difficulty of "demonstrating" that the second spelling was not intentional.

Reverting to *"hopiknsii,"* Mr. Follett suggested that to provide for the emendation of a name because it comprises an unpronounceable combination of letters would be to introduce a highly subjective criterion. One of the members concurred, with the remark that the personal surname "Krska" might be regarded by many people as an unpronounceable combination of letters.

Mr. Follett compared the problem of misspelled names to that of "inappropriate" names, and read Article 32 of the International Rules, which for many years has provided that "A generic or a specific name, once published, can not be rejected, even by its author, because of inappropriateness. Examples: Names like *Polyodon, Apus, albus,* etc., when once published, are not to be rejected because of a claim that they indicate characters contradictory to those possessed by the animals in question." He stated that the trivial name *"texanus"* was applied to a fish of the great Colorado River, possibly in the mistaken belief that it had been collected in the Colorado River of Texas; that one of the best-known fishes of North America, the blackbass, has been known for 150 years by the generic name *Micropterus* (meaning "small fin") because part of a fin of the holotype had been bitten off; and that both of these "inappropri-

ate" names are in constant use, but seldom evoke comment.

Dr. Usinger asked what should be done with emendations established in the literature—which, in the nomenclature of insects, he estimated at about one percent of the described forms. Mr. Follett estimated such emendations in the nomenclature of Californian fishes also at about one percent of the described forms. He suggested that the dislodgment of any one of the established emendations would work no greater hardship than did the dislodgment of the established emendation *"equisetis"* when, after more than a century, it was "demonstrated" that the original orthography (*"equiselis"*) was not a misprint but a variant spelling. He suggested further that this aspect of the problem seemed to present a simple choice between (1) original orthography and (2) usage—and that the question of what constitutes "usage" would seem to be highly subjective.

Dr. Usinger asked whether the definitions of the expressions "erroneous original spelling," etc., as proposed by Hemming (1952, 21, 47) appeared satisfactory. Mr. Follett replied that he considered them so, except that he concurred with the suggestion of Sabrosky (1952, 5) that the expression "erroneous spelling change" should be emended to "erroneous subsequent spelling," for the reasons stated by that author.

Dr. Usinger queried whether, under the rule proposed by Mr. Follett, there would be any need for the expression "valid emendation." Mr. Follett suggested that a "valid emendation" would be one effected by the International Commission.

Mr. Follett emphasized the difficulty of expressing an objective test that would determine (1) what constitutes "evidence" (direct or circumstantial) of error in an original description, and (2) whether there is a conflict within that "evidence."

Dr. de Laubenfels moved that Mr. Follett's suggestion that "The original orthography of a scientific name shall be preserved, unless the International Commission shall emend it" be approved by this body. The motion was seconded by Grace L. Orton. A showing of hands indicated 13 for and 4 against the motion, with one (Mr. Follett) abstaining. A check of SSZ members present, as distinguished from guests and visitors, showed 18.

Dr. Usinger then suggested that the recommendation of Blackwelder, Knight, and Sabrosky, cited above, be recorded as the view of the minority. No action was taken, but Dr. John N. Belkin voiced his disapproval of the International Commission as final arbiter in any event.

5. "The Possible Addition to the *Regles* of Provisions Governing the Naming of Orders and Higher Taxonomic Categories."

The time that remained was spent in discussing, under the leadership of Dr. Usinger, an aspect of this question on which the advice of specialists is now being sought: Should orders, classes, and phyla be defined by types, and if so, should the type be a genus, or a family, or some other category?

Dr. Usinger read sections from a plan proposed by J. Chester Bradley, under which the International Commission would be authorized to prepare master lists of these higher categories and to fix types, presumably at the generic level. Dr. Brittan inquired on what basis the type genus would be selected, whether priority, euphony of name, locality, or some other. Mr. Follett queried the criteria practicable in such matters.

Dr. de Laubenfels favored retention of the *status quo* unless it could be shown that definite advantages would result from any new arrangement. Dr. Usinger thought that the stability which would result constituted a sufficient advantage, and cited the variety of ordinal names in current use for the insects, no two authors seeming to agree upon the list.

Mr. Ferris would restrict the proposal to categories of superfamily and below. He also raised the question as to what would constitute priority in the case of

an ordinal or class name. Dr. Usinger admitted the possibility of priority without types, with usage alone as a guide. Dr. Belkin, however, was of the opinion that priority could not be used without the type system.

With such a variety of opinion expressed, and in consideration of the limited information available, it was decided not to take a vote on the proposal. It appeared, however, to be the consensus of those present (1) that orders should not be defined by the type concept; (2) that no strong opposition to the application of priority was indicated; and (3) that otherwise the recommendations embodied in J. C. Bradley's commentary upon Mr. Hemming's views are generally acceptable.

The papers given in this symposium will be published in the December issue of SYSTEMATIC ZOOLOGY.

Luncheon: The SSZ luncheon arranged by Ivan Pratt of Oregon State College at Corvallis proved to be excellent and most enjoyable. It was attended by 32 persons, including executives of both the national and the divisional AAAS offices. These were Raymond L. Taylor, assistant administrative secretary from Washington; Robert C. Miller, secretary-treasurer of the Pacific Division in San Francisco; and E. B. Babcock, associate secretary of the Pacific Division.

Business Meeting: The first annual business meeting of the Pacific Section of the Society of Systematic Zoology was held in the Memorial Union Tearoom, Oregon State College, Corvallis, Friday, June 20, 1952, following the luncheon.

The meeting was presided over by W. I. Follett, a Councillor of the Society who gave a résumé of the Society's history and that of its unofficially recognized Pacific Section. The minutes of the meeting of the organizational committee held at Claremont, California, December 28, 1951, were read by the provisional secretary, John S. Garth, but approval was deferred on ruling by the chair pending formal organization. There followed the reading of a report on the circularization, last April, of some 234 members of the Society living in the West, which showed that of 112 persons who returned the questionnaire, 108 favored the organization of a Pacific Section, 102 were willing to support it financially, 23 planned to attend the Corvallis meeting, and 76 were interested in attending future meetings of the proposed section.

The following officers were elected: president, John S. Garth; vice-president, Robert C. Stebbins; secretary-treasurer, John N. Belkin; and representative on the Council of the Pacific Division, AAAS, G. F. Ferris.

As his first act in office, the newly elected president entertained a motion for sectionalization, which was made by W. I. Follett. As a result of action taken, a petition will be prepared for presentation to the Council of the Society not later than December, 1952, by its two western members, G. F. Ferris and W. I. Follett, requesting formal recognition of a Pacific Section to include the states of Washington, Oregon, California, Nevada, Utah, Idaho, and the province of British Columbia, with invitations to Hawaii and Alaska. It is hoped that by proceeding without delay the new Section may be sufficiently well established to serve effectively as host to the parent organization when it meets nationally with the AAAS in San Francisco in December, 1954.

Session for Submitted Papers, G. F. Ferris, presiding:

1. EDWARDS, J. GORDON, San Jose State College: "Some Problems Encountered in the Systematic Treatment of a Polytypic Species of Phytophagous Leaf-Beetles."

2. HEDGPETH, JOEL W., Scripps Institution of Oceanography, La Jolla: "On certain fossil 'Pycnogonida.'"

3. BRITTAN, MARTIN R., San Diego State College: "Adaptive Radiation in the Freshwater Fish Family Cyprinidae."

4. DE LAUBENFELS, M. W., Oregon State College: "What is a Species, or, a Defense of *Sphyrapicus ruber*."

5. KISER, R. W., Centralia Junior College, Washington: "Speciation in Cladocera in North American Lakes and Ponds."

6. DURHAM, FLOYD E., Allan Hancock Foundation, University of Southern California: "Variations in the Rodents of the North Rim of the Grand Canyon, Arizona."

This highly diversified program, with subject material drawn from both warm and cold-blooded vertebrates, fresh water, marine, and terrestrial invertebrates, and fossil as well as recent forms, was kept moving by an alert chairman, who limited each paper to its allotted 15-minute period. The highly spirited discussions, which were entered into alike by specialists and non-specialists in the groups concerned, proved that in systematic zoology all disciplines find common ground. The exchange of ideas was highly profitable to all who participated, and the Santa Barbara meeting in 1953 is keenly anticipated.

The Book Exhibit: About 250 recent zoological books were sent from the library of the Society of Systematic Zoology for exhibit in the headquarters lounge in the Memorial Union. Nearly 300 persons visited the exhibit, which was open for five days. The local library and the college science departments cooperated to make this feature a success.

Many persons share responsibility for the success of this meeting. John S. Garth served as provisional secretary of the committee to organize the proposed Pacific Section and was later elected president of the new section. His committee consisted of the following members: J. N. Belkin, J. Hedgpeth, M. A. Miller, I. Pratt, W. T. Edmondson, D. P. Abbott, L. M. Klauber, R. C. Stebbins, J. M. Savage, T. H. Butler (Canada) and A. H. Banner (Hawaii). W. I. Follett organized the symposium and G. F. Ferris organized the session of submitted papers. Ivan Pratt took charge of the local arrangements to the satisfaction of all concerned. He was assisted by Vincent Roth and others on the Corvallis campus.

OUR FIRST PATRON

Members who do not subscribe to SYSTEMATIC ZOOLOGY may not see the announcement in this issue that Captain Allan Hancock has become the first patron of the Society, through his donation of $1000. It is most appropriate that Captain Hancock should be the first, as he has been very active in zoological exploration for many years and has been an enthusiastic supporter of taxonomic research. The Allan Hancock Foundation at the University of Southern California is a fitting memorial to his interest and activity in zoology, paleontology, oceanography, and other natural sciences. The Society of Systematic Zoology is proud to be included among the organizations in which Captain Hancock has taken an interest.

SYSTEMATIC ZOOLOGY is published quarterly by the Society of Systematic Zoology. Its purpose is threefold: To publish, and therefore to encourage the preparation of, contributions on basic aspects of all fields of systematics, principles and problems; to provide a suitable forum for discussion of the problems of the systematist and his methods; and to report as news the other activities of the Society of Systematic Zoology.

Contributions of the following types are solicited: Papers on principles and the applications of principles of wide implication and general interest in any phase of systematics, such as comparative anatomy, zoogeography, paleontology, taxonomy, classification, evolution, or genetics; discussions of methods, specific problems, and activities of systematists; discussions of new books and monographs; and news of systematists, organizations interested in systematics, research and teaching programs, expeditions, collections, meetings, and anything else of interest to systematists.

Major articles should be from 1500 to 10,000 words in length. Discussion articles should not be over 400 words unless of very general interest. Contributors are encouraged to submit line drawings and diagrams to illustrate their articles. Half-tones may be accepted where necessary to the article.

THE PUBLICATION FUND

A Publication Fund was established to help in the initiation of this journal. It consists entirely of donations from the members. It is hoped that it will be a permanent fund for use in other publication programs as well. For this reason further contributions of any amount will be welcomed. At present this fund is restricted to use for SYSTEMATIC ZOOLOGY, under the supervision of the Council.